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SELECTED PAPERS.

State of Medicine in Philadelphia, from the first settlement of Pennsylvania, to the year 1762.

From "An Eulogium on Doctor William Shippen, delivered before the College of Physicians of Philadelphia, March, 1809, *by* DOCTOR CASPAR WISTAR, *one of the Censors.* Philadelphia, 1818.

IF Doctor Shippen was fortunate, in possessing many of the personal requisites, for the undertaking in which he was about to engage; I believe it will appear, that he was equally so as to the particular state of medicine in the city when he commenced it. To render this evident, I will quit, for a short time, the proper subject of my memoir, and take a general view of the successive practitioners of medicine in Philadelphia, in the order of time in which they lived. The first physicians were two Welsh gentlemen, who came with the original settlers, in 1682. One of these, Thomas Wynne, is said to have practised with great reputation in London; but he died within ten years after his arrival, and was much engaged in political business during his residence here. He therefore does not appear to have given any particular direction to the medical profession; but his pupil, Edward Jones of Merion,

on the west side of Schuylkill, a man of great worth, educated a son in his own profession. This son continued the professional education to his nephew, our respected predecessor, Doctor Cadwallader; and he extended it to our late beloved vice-president, Doctor John Jones. The other original physician was Griffith Owen, who arrived in the prime of life, and lived here many years. This gentleman appears to have done the principal medical business of Philadelphia, and was highly respected for his professional talents, integrity, and spirit. If the circumstance had not occurred so often, that we are become familiar to it, we should be greatly surprised, that a practitioner of this description, should pass through life, without committing to writing any acount of the diseases which occurred; or of the method of treating them which he found most efficacious: such, however, seems to have been the fact. He died in 1717, about the age of seventy; and left a son, who practised medicine here some time after his father's death. I have not heard of any physicians of respectability in Philadelphia, but these, from the first settlement, until the arrival of Doctor John Kearsly senior, and Doctor Græme; the latter of these gentlemen came here in the year in which Doctor Owen died.—Doctor Kearsly probably arrived some time before. I believe Kearsly came in quest of a professional birth. He was, for a long time, a very industrious practitioner, both in medicine and surgery; but, like Doctor Owen, he does not appear to have committed to paper any of his professional observations. Although he made no exertions to increase the progress, either of medicine or of general science, yet he was not deficient in public spirit. Philadelphia was more indebted to him than to any other man, for that respectable edifice, Christ Church; and, by will, he founded and endowed a hospital for poor widows. He educated our late respectable President, Doctor John Redman, and the worthy Doctor John Bard, of New York. His cotemporary, Doctor Græme, came to Philadelphia from Great Britain, with the governor, Sir William Keith; and of course, had an advantageous introduction. He was about thirty years of age, when he arrived; a man of excellent education and agreeable manners; and was therefore much employed as a practitioner, and greatly confid-

ed in by his fellow citizens; but, like Kearsly, the promotion of science does not appear to have occupied much of his attention. He exhibited, however, to the people of this part of America, an object, which was then entirely new to most of them; a very large farm, Græme Park, completely surrounded with hedge, and embellished with great taste and elegance.

Although Philadelphia, in the early part of the career of these gentlemen, does not appear to have held out many advantages in medical science, yet two young men, who received all their education here, made a very respectable proficiency. Doctor Shippen, senior, whom I have already mentioned, was one of them; and Doctor Lloyd Zachary the other. Doctor Zachary was very nearly related to those distinguished citizens, Isaac and Charles Norris. He probably commenced the practice of medicine between 1720 and 30, and died in the year 1756; in the middle period of life, greatly and most deservedly respected. These two last mentioned gentlemen may be considered as belonging to the medical period in which Doctor Shippen's undertaking commenced; for, although Doctor Zachary died some years before, yet he acted an important part in several of the transactions which distinguish this time. He was one of the founders, both of the College and of the Hospital, and a very liberal contributor to each.

We have now come to the time of those physicians, who were in practice when Doctor Shippen arrived. The eldest of them was our eminent predecessor, Thomas Bond. This celebrated physician and surgeon was a native of Maryland; and studied there under Doctor Hamilton, a very learned practitioner. Doctor Bond travelled in Europe, and spent a considerable time in Paris, were he attended the practice of the Hotel Dieu.

He commenced the practice of medicine in this city, about the year 1734, and soon attracted the public attention. He was amongst the founders of the College and Academy, and one of the most active managers of the Pennsylvania Hospital, at its commencement; and with his brother, Phineas Bond, and Lloyd Zachary, made the first offer to attend that institution, as physicians and surgeons. He was an active officer of the Philosophical Society, from its first establishment; and ap-

pears, by the old records, to have been a member of a smaller society, instituted in 1743; of which Doctor Benjamin Franklin, William Coleman, John Bartram the botanist, and Doctor Phineas Bond, were also members. This society, in 1768, united with another which had also been a long time in existence; and the united bodies then assumed the name and form which they now employ. His brother Doctor Phineas Bond, who was several years younger, was also educated in Maryland; but he had studied medicine upon a most extensive scale; for he passed a considerable time at Leyden, Paris, Edinburgh and London; and was not only well disposed to promote, but well qualified to judge, of every undertaking for the improvement of his country. In conjunction with the much respected Thomas Hopkinson, he originated the scheme of the College, now the University of Pennsylvania. Not practising surgery, he moved in a different line from his brother; but no medical man in Pennsylvania ever left behind him a higher character for professional sagacity, or for the amiable qualities of the heart.

Doctor Thomas Cadwallader, who has already been named, comes now to be again mentioned. To complete his education, he went to England; and, I believe, also to France. In England he studied Anatomy under the celebrated Cheselden; and, according to correct information, I find, that on his return to Philadelphia, he made dissections and demonstrations, for the instruction of the elder Doctor Shippen, and some others, who had not been abroad. This, probably, was the first business of the kind ever done in Pennsylvania.

From the place of its performance, the back part of the lot on which the Bank of Pennsylvania now stands, I suppose, that the Anatomy of that day, as well as of the present, enjoyed the honourable protection of literature; and that the dissections were made under the auspices of the most profound scholar of Pennsylvania; the President, James Logan, founder of the Loganian Library.

Cadwallader made equal steps with the Bonds, in promoting the interests of the Hospital, College, and Philosophical Society; and always had a great share of well-merited influence with his fellow citizens.

Doctor Shippen, senior, although his opportunities had been very different, supported, at that period, equal rank in his profession with these gentlemen; and also paid great attention to the public objects I have mentioned.

The fifth man of this class, was our late worthy President, Doctor John Redman. He first studied in Philadelphia, under Kearsly; and then, in quest of improvement, visited London, Edinburgh, and Paris; and graduated in Leyden, a short time after the death of Boerhaave. His professional education was therefore of a superior kind. He was much employed as a practitioner, soon after his return to the city; and was long a faithful physician of the Hospital. He was also a trustee of the College, and a member of the Philosophical Society; but he had very little leisure, either for literary or philosophical pursuits; for the number of his patients was immense.

The youngest physician of this class was Doctor Cadwallader Evans. This gentlemen had been one of the first pupils of Thomas Bond, but completed his medical education in England. He was a descendant of a much venerated early settler; and, with the rest of his family, retained the virtue and character of their ancestor; having a great share of public spirit as well as of professional worth.

Such were our predecessors. From this short sketch of their history, I think it may be inferred, that they effected a most important change in the literature and science of their country; and that Doctor Shippen could have had no better men for patrons.—When we consider the state of the public mind, as evinced by the establishment of the Hospital, College, and the Philosophical Society, soon after,—when we reflect, that the experiments of Franklin, on the lightning of heaven, must have prepared our fellow citizens for the investigation of every other part of nature, and that the sacred principle of toleration, established by our great founder, may also have operated in favour of Anatomy;—we must conclude, that Shippen could not have fixed upon any part of the new world, which, at that time, was more promising than Philadelphia. He was not disposed to neglect any of these advantages.

On the use of the Prussic (Hydrocyanic) Acid in the Treatment of certain Diseases of the Chest, and particularly in Phthysis Pulmonalis. By Dr. Magendie. *(Read at the Royal Academy of Sciences of Paris the 17th November, 1817, and communicated exclusively to the Editor of the Journal of the Royal Institution.)*

[From the Journal of Sciences and the Arts, No. 8. for 1818.]

My Dear Sir,

The very distinguished author of the subjoined memoir, well known for his zeal in promoting physiological science, has transmitted the original to me, immediately after its having been read before the Royal Academy of Sciences at Paris, with a request that I would translate it, and publish it exclusively in the Journal of the Royal Institution. The well merited reputation which this Journal has gained among the learned of the continental nations, for its candour, impartiality, and the originality of some of its valuable Papers, will explain this preference in my friend Dr. Magendie; and I feel happy in the opportunity of saying, that it is his intention, and that of many other eminent characters in Paris and in Italy, with whom I have conversed on the subject, occasionally to favour the Journal with their original communications.

On the subject of the following pages I might descant a little, were it not superfluous to repeat what the author himself has so clearly stated. Its importance is too evident to need any recommendation. But you will perhaps allow me to say, that having been present at some of the experiments; conscious as I am, from actual examination during my residence in Paris, of the truth of many of the facts brought forward; and enjoying the pleasure of intimately knowing the author, to whose sagacity and talents I shall be happy on all occasions to do justice, I cannot hesitate in recommending Dr. Magendie's paper to the earnest attention of all my medical brethren.

Yours truly,

A. B. GRANVILLE.

8, *Saville Row*,
December, 1817.

PHYSIOLOGICAL experiments are of the utmost importance in the practice of medicine. It is by means of them, that substances used as medicines on mere hypothetical principles are justly rejected, and the really active remedies better known, or more beneficially employed, by varying their proportions and mode of administration. Another great advantage resulting from physiological experiments, is the discovery of new remedies, which the physician is enabled to make by their assistance; and by directing his attention to substances already known, but either neglected or seldom used, as well as to those preparations, which modern chemistry is daily bringing before us, and which, in the hand of an able and experienced practitioner, may become particularly useful to mankind.

It was on account of this firm persuasion, that I have ventured, at different periods, to call the attention of the Royal Academy of Sciences to the poisonous and medicinal qualities of the *Upas tieuté*, the *nux vomica*, the *supertartrate of antimony and potash*, and the *ipecacuanha*. The favourable reception which my researches have met with, have encouraged me to proceed farther; and, I shall have the honour this day of laying before the Academy, the result of my inquiries respecting the action of the prussic or hydrocyanic acid on the animal system, and the good effects which may be derived from it in the treatment of several diseases.

The prussic acid, discovered in 1780, by Scheele, was soon afterwards reckoned amongst the poisonous substances; an opinion which was confirmed by the experiments made, both in Germany and France, by Messrs. Coulon, Emmert, Robert, Orfila, &c.—From their experiments, and from some which are peculiarly my own, it results; 1st, that the prussic acid, whether liquid or in the state of vapour, is injurious to the life of all animals, and in many cases, to that of vegetables; 2d, that death produced by this acid is much more instantaneous, from the circulation becoming more rapid, and the lungs more distended; 3d, that it acts on warm blooded animals by destroying their sensibility and the contractility of the voluntary muscles; and would act in a similar manner on man, where the dose given is greater than what will be hereafter mentioned.

It cannot be denied, therefore, that the prussic acid is a very active poison; and yet all the experiments I have just now alluded to, have been made with the prussic acid of Scheele, which contained a very considerable quantity of water, and was consequently very weak. From this circumstance it will be easy to conceive, that the effects of the *pure* acid, prepared according to Gay Lussac's directions, must need be much more energetic. Its activity, indeed, is really frightful, even to those who are accustomed to witness the effects of poison. Of this the Academy shall judge by the following facts.

Experiment 1. The extremity of a glass tube which had been previously dipped into a phial containing some pure prussic acid, was immediately plunged into the throat of a strong dog. The tube had scarcely come in contact with the tongue, than the animal made two or three long and rapid inspirations, and fell dead. No method we could devise, enabled us, afterwards, to trace the smallest sign of sensibility in the muscular organs of this animal after death.

Experiment 2. An *atom* of the acid was applied to the eye of another strong dog; the effects were as sudden and as fatal as in the preceding experiment.

Experiment 3. A drop of the acid diluted with four drops of alcohol, were injected into the jugular vein of a third dog. The animal fell dead, that instant, as if struck by *a cannon shot or by lightning*.

In short, the pure prussic acid, prepared according to M. Gay Lussac, is without doubt, of all the known poisons, the most active and the most promptly mortal. Its deleterious and powerful influence permits us to believe, what the historians have related, of the criminal talent of Laucustus; and render the accounts of those extraordinary and sudden cases of poisoning, so frequent in the annals of Italy, less marvellous and incredible.

I must not omit to say, likewise, for the sake of those who should feel disposed to make experiments with this substance, that the utmost caution is necessary; as it is impossible to breathe its vapour without feeling the most dangerous effects. Owing to some neglect on our part, in this respect, we have at

one time experienced the most excruciating pain in the chest, accompanied by a feeling of insupportable oppression, which lasted some hours.

It might appear from all this, that the pure prussic acid, in the hands of one intent on murder, would become the instrument of crime, without fear of detection; but the public may make themselves easy on that score. More nicety of manipulation, and dexterity in the operations of chemistry is required to procure this acid, than falls to the lot of common people; and even when properly prepared, it is almost impossible to preserve it in its state of purity, as I have ascertained by direct experiment. It is spontaneously decomposed at the ordinary temperature of the atmosphere, and then loses, in a short time, all its obnoxious qualities. Besides, though the prussic acid produces death, without any visible alteration of the animal organs, it is easy to detect a case of poisoning by it; for the body will exhale, during several days, a strong smell of bitter almonds.

Poisonous as it is, there is no doubt, but that the prussic acid may, when properly diluted with water, be used as a medicine with safety. We know from the experiments which Mons. Coulon made on himself, that it may be given to the dose of sixty drops without producing any very serious inconvenience. Besides, the pretty frequent use made in medicine, of the laurel water, in which the prussic acid enters, as a component principle, proves that it may be introduced into the stomach when properly diluted. Nothing therefore, shews any impropriety in its use as a remedy; a circumstance which has already induced some French and Italian physicians to give it in various disorders. If their success has not been equal to their expectations, it is because they did not seem sufficiently aware of its mode of action on the animal economy; and without this knowledge it is impossible to make a right use of any new remedy whatever.

In studying the phenomena of poisoning by prussic acid, I have often observed, that animals, in which no trace of sensibility, or muscular contractility could be found, would often continue to breathe for several hours, freely; while their circulation, though much accelerated, remained apparently unal-

tered. These animals indeed might have been said to be dead with regard to their external functions, yet still enjoying life, through their nutritive faculties.

This property of extinguishing the general sensibility without any ostensible injury to the respiration and circulation, the two principal functions of life, induced me first to believe, that the prussic acid might be advantageously used in cases where the disease seemed to owe its origin to a vicious augmentation of sensibility. From that moment I determined to use it whenever any such case should offer itself to my attention.

About three years ago I was consulted in behalf of a young lady, aged 27; who, for the space of eighteen months, had been distressed by a dry short cough, which became stronger in the evening and in the morning. Alarmed at these symptoms, which seemed to indicate an affection of the lungs, her friends took the advice of several of the most distinguished medical practitioners of the capital, who all prescribed the usual remedies in such cases, but without success. I ordered her six drops of Scheele's prussic acid, prepared by Pelletier, diluted with three ounces of a vegetable infusion; to be taken by spoonfuls every two hours. The following day the cough had considerably diminished, and it disappeared entirely on the fourth.

The cough however, having made once more its appearance six months afterwards, I repeated the same remedy with an equal success.

Since then I have had repeated opportunities, but chiefly with young ladies, to employ the prussic acid in cases of nervous and chronic coughs; and have always obtained the greatest success, without having observed any inconvenience from it. In no case have I gone beyond the dose of *ten* drops taken at intervals during twenty-four hours, and diluting it with several ounces of some fluid vehicle.

Very lately I have succeeded in calming by this same means a convulsive cough, with which an elderly lady of a nervous temperament had been greatly affected, and which for six days previous to my seeing her, had come on by alarming fits, depriving her of all rest. I was so much the more willing to

adopt in this case the use of prussic acid, as the patient could take neither opium nor any preparation of poppies without being grievously incommoded.

After thus having ascertained the efficacy of the prussic acid in the treatment of dry convulsive cough, I thought it was indispensable for me to inquire whether the same means might not be employed with success to combat the cough and other symptoms which overpower the unhappy consumptive—and whether it would not influence, or even suspend the progress of pulmonary consumption.

The result of my trials has been favourable with regard to the first of these conjectures; and on fifteen persons, affected with phthysis, who had been placed under my care for the last three years, I have constantly found that the use of the prussic acid, given in small but repeated doses, diminished the frequency of the cough, moderated, and rendered more easy the expectoration, and lastly, procured the patients some sleep at night without any colliquative sweats. Those who are accustomed to follow the march and progress of phthysis, and witness the sufferings without number, by which individuals attacked by this terrible malady are overpowered, will easily appreciate the real benefit of this success.

Since the beginning of the month of August last to this day (November) I have had many opportunities of studying the effects of prussic acid on a great number of phthysical patients at the hospital of *la Charité*. Mons. Lerminier, physician to that hospital, in which such diseases are very frequent, has, at my request, agreed to administer the prussic acid in about twenty cases, at the dose of four drops properly diluted with water.

The greater number have shewn evident signs of amelioration, and some seem much better at this moment. The cough is considerably diminished. The expectoration has become easy, and sleep came to shorten their sufferings. These improvements became more evident, where the disease was in an incipient state; a circumstance which is not difficult to explain, when it is considered, that the lungs are in a state of disorganization, in the second, and above all, in the third stage of consumption.

Yet as I wish to state merely, in this place, the *exact* effects

of the prussic acid, I must avow, that amongst the patients of *la Charité*, who have used it, some, whose disease was near its end, did not derive any very sensible benefit from it; and that in two instances, in which the patients had taken the acid at too short intervals, they experienced some headache, and a kind of vertigo which lasted some seconds. In a third case it was feared that the acid had proved injurious.

A young man aged 29 was admitted into the hospital towards the end of September last with all the symptoms of a a confirmed phthysis.* The acid was given in the dose of six drops only. The cough diminished, the second day, but the oppression increased; the third day it became suffocating, though the acid had been discontinued the preceding day; and the patient fell into a sort of insensibility which terminated in a few hours in death. This could not have failed to raise some suspicions against the remedy, if the examination *post mortem* had not clearly shewn, that the fatal termination of the malady, was owing to an immense quantity of serous fluid found in the left side of the thorax; the heart being thrown to the right, and touching the ribs of that side. Neither the stomach nor the body exhaled any prussic smell. The disease in this case was in its second stage; but it was evidently not the principal disease by which the patient was affected; a circumstance which will account for the absence of all sort of beneficial results from the prussic acid.

In my private practice I never observed any bad effect to result from the action of the prussic acid given as I directed, which may be accounted for by considering the great care and attention which a patient in the bosom of his family receives at every moment; the case being necessarily different in regard to patients in even the best regulated hospitals.

From all that precedes, I think I am warranted in concluding, that the prussic acid, given in small doses, mixed with a certain quantity of water, may be advantageously employed as a palliative treatment of consumption, with a view of calm-

*Contrary to the established practice of Mons. Lerminier, the chest was not explored by means of percussion, as is generally done with all the patients who enter at *la Charité*. This prevented the discovery of an old inflammation of the pleura which affected the left side of the chest.

ing the cough, facilitating the expectoration, and procuring sleep; and that as such it must be considered as the first among the substances usually employed for similar purposes; as it does not seem to excite, like the opiates, any colliquative sweat.

It still remains to inquire, whether, by the assistance of the prussic acid and of its marvellous activity, we might not hope to render the march of phthysis more slow, and even to cure it. But these questions, in themselves so important, on account of the too fatal prevalence of the malady, cannot be decided by a small number of facts and experiments. They ought, on the contrary, to be multiplied as much as possible, taking at the same time into consideration all the circumstances which might influence the results; and divesting ourselves of all sort of prejudice.

I am continuing my experiments on this subject, conjointly with M. Lerminier, at the hospital of *la Charité;* where, from twenty to thirty consumptive patients are habitually received; and I am in hopes to lay before the Academy, in the course of the next year, some facts worthy of their attention.

Some may consider it as a mark of temerity in me to suppose that phthysis may be cured, when so many very eminent authors look upon it as absolutely mortal. But admitting that phthysis be incurable,with reference to all the substances hitherto employed, and the experiments hitherto made for that purpose, in the true spirit of truth and investigation, the same mode of reasoning cannot hold good with regard to new substances, remarkable for the energy of their action on the animal economy; besides, ought not the physician to direct his attention to the cure of such diseases as *phthysis*, *cancer*,&c. in preference to merely varying the treatment of such diseases as, from peculiar circumstances and their nature, terminate always happily, whatever be the remedy suggested by fashion or caprice?

I shall now relate two cases in which there is reason to presume that phthysis was checked in its progress by the use of prussic acid.

CASE I.

A lady from Lyons, now residing in Paris, of a constitution

eminently bilious, after having experienced several misfortunes, was, in 1814, attacked by all the symptoms which characterise phthysis in its first stage. Circumstances not allowing her to attend to her health, she neglected it, until within the first months of 1815, when the disease having made great progress, she consulted me: I found her labouring under all the symptoms of the second stage of tuberculous consumption, with a cough returning incessantly, and a slow continued fever preying upon her and undermining her existence. The prussic acid was recommended, and taken at the dose of from six to ten drops in twenty-four hours, diluted. This acid had been prepared by Mons. Planche, by a new process which I shall hereafter describe. The remedy was continued for about two months. From the first day the cough diminished, the patient slept, and without pushing the dose higher than ten drops in the 24 hours, all the symptoms of the disease disappeared, the breathing became natural, the cough, expectoration, and sweats ceased. In short, the lady was perfectly cured, and has never since experienced any symptoms, which indicate the least disposition to a relapse. Her lungs only have become very sensible to the influence of atmospherical variations.

Must we conclude from this fact, that a consumption in its second stage has been cured by the prussic acid? I am far from thinking so; for I know with how much reserve we ought to draw any general and positive conclusion. Yet such as it is, and with all the importance attached to it, I submit the case to the practitioners, who take an interest in the progress of science.

CASE II.

An English lady aged 28, tall, and of a weak constitution, with a chest transversely ample, but narrow, was frequently subject to colds from her infancy. Last year, while crossing over from France to England, she was attacked by an inflammation of the lungs, with acute pain of the left side of the thorax, and spitting of blood. Bleeding, blisters, and all the means usually adopted in such cases, were had recourse to, and she got well; but she continued to suffer from a short dry cough, of no great intensity during the day, but which became

greatly exasperated in the night. Several means were employed in England to check it, with no success. Believing that the climate of France might prove more beneficial to her health, she returned to Paris about four months ago. In spite of the fineness of the season, and residence in the country, the cough made considerable progress; she became uneasy and alarmed; and I was sent for about the middle of September last; when, after a mature examination of all the preceding circumstances, and her actual state, I did not hesitate in considering her as labouring under the first stage of pulmonary consumption. I prescribed the prussic acid, prepared by Mr. Planche, in the dose of 8 drops in three ounces of water in 24 hours. She has continued it ever since, and is at this day taking it. Her cough has nearly vanished altogether; she has gained considerable *en bon point;* and she considers herself at this moment as quite recovered.

Far be it from me to suppose that this is another instance of pulmonary consumption cured by the prussic acid. Yet it must be confessed, that if examples like the two preceding ones, were to become numerous, nothing could then prevent us from hoping, that we may at last have found a substance capable of arresting the progress of one of the most desolating maladies by which humanity is afflicted.

I shall conclude with a few words on the mode of preparing the prussic acid. If it be prepared in the way directed by Scheele, we run the chance of obtaining it at various degrees of concentration—a circumstance which ought by all means to be avoided. M. Planche, one of the most distinguished *pharmaciens* of Paris, follows a method which renders its preparation less uncertain. Instead of drawing one-fourth of the produce by distillation, as the illustrious Swedish chemist first suggested, he stops the operation, as soon as $\frac{1}{6}$ of the produce has passed into the receiver. He next rectifies the liquid thus obtained by means of a gentle fire, over $\frac{1}{200}$ of carbonate of lime, and draws off $\frac{3}{4}$ only of the whole by distillation.

Thus prepared, the prussic acid is certainly far from possessing the activity of that prepared by Gay Lussac; but it has more energy than that of Scheele, and above all, it has an *invariable* energy; since, according to M. Planche, it is always

at the same degree of concentration. It also affords one other advantage, that of being susceptible of preservation, if kept in a cool place, equally removed from the influence of air and light.

I have also employed in many cases the acid prepared according to Scheele's method, and have not been struck with any difference in its action, such as the theory of chemical doctrine seems to suggest, whenever I have taken care to get it at the houses of our best chemists.

The observations in this Paper are,

1st. That the pure prussic acid is a substance eminently deleterious and altogether unfit to be used as a medicine.

2dly. That the prussic acid diluted with water is beneficial in cases of chronic and nervous cough.

3dly. That this same acid may be useful in the palliative treatment of pthysis, by diminishing the intensity and frequency of the cough, and in procuring sleep, &c.

4thly. That there is reason to hope, that this same substance may become advantageous, as a curative treatment of pulmonary phthysis, particularly when in an incipent state.

Some further Observations on the Use of the Colchicum Autumnale in Gout. By Sir E. Home, Bart. V. P. R. S.

[From the Philosophical Magazine, for December, 1817.]

I laid before the Society, some experiments and observations in favour of this medicine acting upon the gout through the medium of the circulation, and not by its effects directly upon the stomach and intestinal canal.

The object of the present paper is to show, that the infusion throws down a deposit, the separation of which does not appear to diminish the specific effects upon the gout, and renders those upon the stomach and intestines milder than when the deposit is taken along with the infusion.

The bulb of the *Colchicum autumnale* contains a certain quantity of extractive matter, and a large portion of mucilage,

both of which are taken up by the wine, in the first instance: when the strained liquor is allowed to stand, a considerable deposit almost immediately takes place.

In the first trials that were made with this medicine in St. George's hospital, it was natural to inquire whether this deposit contained any medical virtues: and upon trials frequently repeated, it was found to have none.

This led to the opinion that the extractive matter suspended in the wine, was alone the active part of the medicine; and not only the first deposit was inert, but also that which from time to time was afterwards found to take place.

Of this opinion I was led to entertain considerable doubts, in consequence of having found upon one occasion, in which I took half a bottle of the *Eau Medicinale* which had been poured off without shaking the bottle, that the sensible effects were very mild; those produced by the other half, in which the deposit was mixed, were unusually severe, the nausea being greater, and a greater number of stools being produced.

These doubts were much strengthened, when I found that the effects of the *Eau Medicinale* are more violent upon many stomachs than those of the vinous infusion of the *Colchicum;* which probably arises from the *Eau Medicinale* being kept in small bottles, in consequence of which all the deposit that takes place is given along with the infusion, while the vinous infusion of *Colchicum* being kept in large bottles, the deposit falls to the bottom. If such deposit increased the powers of the medicine in counteracting the symptoms of gout, it would be unnecessary to prosecute this investigation further, since it would be absurd to diminish the violence of a medicine, if, by so doing, its efficacy is to be diminished in an equal degree.

To ascertain this point, I gave sixty drops of the vinous infusion of *Colchicum*, in which there was no deposit whatever, to a man labouring under a severe paroxysm of gout, to which he was a great martyr, and whose paroxysms were usually of several weeks continuance; he was sixty years of age.

The medicine was exhibited on the 17th of January 1817, his pulse being 115. In half an hour he had slight nausea, which soon went off. In five hours, a profuse perspiration came on, and the pain of the gout entirely subsided, leaving a sore-

ness in the parts that had been affected. In twelve hours the bowels were gently moved, his pulse 105 and irregular; in fourteen hours his bowels were acted on a second time; in nineteen hours his pulse was 92, and natural; in forty-eight hours he was quite well, and has continued so a period of more than three months.

The result of this case satisfied me that the infusion contained the specific remedy for the gout, and that the deposit is not necessary for its removal.

This rendered it probable that, where the deposit is taken along with the infusion, its solid form prevents it from being carried into the circulation of the blood, and it remains in the stomach, producing more or less mischief in that viscus, without being any way concerned in driving away the disease for which the medicine was exhibited; in this respect resembling many of the salts of mercury, which irritate the bowels, without relieving the symptoms of the venereal disease.

I explained these opinions to Mr. Gatcombe, who gives me his assistance in my professional pursuits, and requested him to investigate this subject.

To do this more completely, he began by repeating the three experiments detailed in my former paper, substituting the *Eau Medicinale* for the vinous infusion of *Colchicum*, so as to determine with more precision whether they are or are not the same medicine.

Exp. 1. Thirty drops of the *Eau Medicinale* with the deposit were injected into the jugular vein of a dog: the effects were the same as in my experiment with the same quantity of the vinous infusion of *Colchicum*, only the animal was two hours longer in recovering from them, and was purged for nine hours afterwards.

Exp. 2. Sixty drops of the *Eau Medicinale* were given by the mouth to the same dog: the effect was less than in my experiment with the vinous infusion of *Colchicum* exhibited in the same quantity: this arose from a very copious evacuation of urine having been produced.

Exp. 3. One hundred and sixty drops of the *Eau Medicinale*, injected into the jugular vein of a dog, produced rather more violent effects than in my experiment with the same

quantity of vinous infusion of *Colchicum*; the animal died in six hours, and after death the appearances of inflammation in the bowels were more violent, approaching to mortification.

Mr. Gatcombe having found so exact a similarity in the effects of the two medicines, in these trials, I requested him to make the following comparative experiment on the effects produced upon the stomach and bowels by the *Eau Medicinale*, in which there is a deposit, and the vinous infusion of *Colchicum*, in which there is none.

Exp. 4. One hundred and sixty drops of the *Eau Medicinale*, taken by the mouth, produced the same effect, and left the same appearances of death, as when that quantity was injected into the vein, only that the animal lived nine instead of six hours.

One hundred and sixty drops of the vinous infusion of *Colchicum* were given to a puppy of the same litter; they produced vomiting, purging, and a great flow of urine; but the animal very soon recovered.

Two hundred drops of the same infusion, after an interval of several days, were given to the same dog, and the effects were the same; the dog had become much improved in his looks and condition.

Three hundred drops, after an interval of several days, were given to the same dog: effects, corresponding with those of one hundred and sixty drops of the *Eau Medicinale*, were produced. The dog died in nine hours, and the appearances of inflammation after death were of the same kind, but not nearly so extensive.

From these experiments, the *Eau Medicinale*, with the deposit, produces double the irritation on the coats of the stomach and intestines, that is brought on by the vinous infusion of *Colchicum:* this probably arises from the local inflammation brought on by the deposit, upon the internal membrane of these viscera.

To determine as nearly as possible the effects of the deposit, when applied in a solid form to the coats of the stomach and intestines, the following experiment was made.

Exp. 5. Six grains of the deposit of the vinous infusion of *Colchicum* were given to a dog, in bread and milk; in three

hours it produced vomiting and purging, which lasted twenty-four hours; during the latter part of that time, there was blood in the stools, as well as in what was brought up from the stomach.

I wished to repeat this experiment with the deposit from the *Eau Medicinale*, but found, in bottles that had been kept seven years, the wine had become vapid, and, in this decomposed state, the acrid part of the deposit had been taken up again; so that in twelve bottles, containing different quantities, only five grains could be procured, which was quite inert.

Being at a loss to know whether the extractive matter deposited from the infusion is in reality more acrid to the stomach than that suspended in it, or the circumstance of its being applied in a solid form renders it so, I requested Professor Brande to acquaint me, if it could be the effect of any chemical decomposition having taken place.

He favoured me with the following explanation, which is highly satisfactory: "There are certain vegetable bodies, which, when infused in water or diluted spirit, furnish a solution which lets fall a sediment, in which their activity, as purgative medicines, chiefly resides; this is remarkably the case with the wild cucumber or *Elaterium*. The sediment is a very drastic purge; the part that remains dissolved is comparatively mild in its operation upon the bowels." This explanation of Professor Brande applies to the *Colchicum*, and we are now enabled to separate the purgative qualities of the vinous infusion of *Colchicum* and *Eau Medicinale*, from those which prove a specific for the gout, in the simplest possible manner, by keeping them in large bottles, instead of small ones, and not going too near the bottom.

It also explains what is asserted by Prosper Alpinus*, that the Egyptian women eat the fresh bulbs, that they may grow fat; an effect which was found to take place in the dog, while the dose was confined within such limits as not to act too violently upon the bowels.

The bulbs of the Egyptian *Colchicum*, when long kept, weigh one drachm each; on being steeped in water they dou-

* Hist. Nat. Egypt. pars 1. lib. 3. cap. 14.

ble their weight; so that the quantity of extractive matter contained in two or three recent bulbs, while combined with the mucilaginous matter, of which the bulbs are principally composed, is not likely to be sufficient to do more than act as a brisk purgative, the occasional use of which tends to make people grow fat.

Since this paper was read, the patient who is mentioned as having had the gout in January, has had another attack: it came on the 10th of July, and was removed in the same manner as the former, by the same dose of the medicine. The President of the Society also, convinced by the evidence contained in this and the former paper, that the *Vinum Colchici*, in which there is no deposit, must be a less hurtful medicine than the *Eau Medicinale*, thought it a duty to himself and the public to make trial of it; and on the 20th of July, when the gout in his left hand and the whole of the joints of that side of the body was very severe, allowed me to give him ninety drops of the *Vinum Colchici*, and found that the symptoms of gout were sooner and more completely removed than they ever had been by the *Eau Medicinale*, of which he has an experience of seven years; having taken it regularly ever since the 17th of February 1810, and during that time kept a regular account of the doses, their effects, and the intervals between them.

On the Pellagra, a disease prevailing in Lombardy.

BY HENRY HOLLAND, M.D. F.R.S.

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A SHORT residence in the north of Italy, at two successive periods, has given me the opportunity of making some observations on the singular variety of disease, which forms the subject of the following paper. The extensive and destructive progress of the Pellagra in Lombardy during the last twenty or thirty years, has rendered it an object of equal interest to the physicians and governments of the country, and has led even to the suggestion of legislative means for controlling

the evils which it has inflicted on this portion of Italy. Many of the remarks on the subject which I shall take the liberty of offering to the Society, are founded on my own observations in the hospitals at Milan and elsewhere. The remainder have been furnished me, either by conversation with different Italian physicians; or by the perusal of two or three of the several treatises, which have been composed upon the pellagra. From the latter I have derived in particular, much of what I shall have to say, as to the locality, progress, and causes of the disease: these being the points, which, for obvious reasons, are least accessible to the observation of the traveller.

The pellagra has derived its name from an affection of the skin, which is one of the earliest and most conspicuous symptoms of the disorder. Though there has been some controversy on the subject, yet I do not hesitate in classing the disease amongst the impetigines, and perhaps even a generic distinction under this class might be warranted by the difference between its symptoms, and those of the leprosy or scurvy, to which it seems to be most nearly allied. Alibert, in his valuable work on the Diseases of the Skin, has described it under the name of the Ichthyosis Pellagra;* but from my own observation of the disorder, I do not feel entirely satisfied with this name or classification. The account I am about to give of its appearances may afford some means of judging as to this point; and I may remark, meanwhile, that the question of nosological arrangement, is one of comparatively small importance, either to the diagnosis or treatment of the malady.

The pellagra, as an endemic disease, prevails chiefly in the provinces of Lombardy, lying between the Alps and the Po. This country may be briefly described as a vast surface of alluvial plain, little elevated above the sea; but rising on its northern side into chains of hills, which intermediately connect it with the Swiss and Tyrolese Alps. From the long, narrow, and profound lakes among these hills, the numerous rivers issue, which flowing southwards to the Po, and giving their waters into a number of artificial channels for the purposes of irrigation, maintain that extraordinary fertility of soil,

* Alibert, Description des Maladies de la Peau; page 175.

for which the plains of Lombardy have long been celebrated. The principal objects of cultivation on these plains, besides the vineyards extensively spread over their surface, are maize, rice, and millet. In some districts, and particularly between the rivers Adda and Ticino, the pastures are extensive, and yield a considerable produce of milk, from which the Parmesan cheeses are made. The hilly country just noticed, on the northern side of Lombardy, is less productive, and cultivated on a more limited scale. The vallies, however, intervening amongst these hills, are of great fertility, yield a considerable quantity of grain, and much wine from the numerous vineyards to which they give shelter.

The district which appears to have suffered most from the ravages of the pellagra, is that which formerly constituted the Duchy of Milan, and particularly the Alto-Milanese, or that portion of country lying up towards the hills between the Lago Maggiore and the Lago di Como. It was in this part of Lombardy that the disease first became an object of medical attention; and some time elapsed before it was described as appearing in the Venetian provinces, and near the shores of the Adriatic Sea. At the present time it has unhappily been recognized as increasing in every part of Lombardy; as well on the plains, as among the hills which rise on their northern border towards the Alps. I have had the opportunity of observing it also in the province of Friuli, the district which intervenes between the foot of the Carinthian Alps, and the northern shore of the Adriatic Sea.

Before alluding to the probable causes which determine this locality and extension of the disease, it will be desirable to relate the symptoms which characterize it in the most ordinary form of its occurrence; though these, I may remark in the outset, display frequent varieties, and are often complicated with other characters of disease. The pellagra is a malady confined almost exclusively to the lower classes of the people, and chiefly to the peasants, and those occupied in the labours of agriculture. Though there are doubtless exceptions to this statement, yet the facts it includes are so general, as to render it of importance in the history of the disease. The first distinct appearance of the malady is under a local cutaneous form;

preceded, however, occasionally by languor, debility, and other indications of a general cachectic state of body. The local symptoms very generally show themselves in the first instance, early in the spring, at the period when the mid-day heat is rapidly increasing, and when the peasants are most actively engaged in their labours in the fields. The patient perceives on the back of his hands, on his feet, and sometimes, but more rarely, on other parts of the body exposed to the sun, certain red spots or blotches; which gradually extend themselves, with a slight elevation of the cuticle, and a shining surface, not unlike that of lepra in its early stage. The colour of this eruption is a somewhat more obscure and dusky red than that of erysipelas: it is attended with no other uneasy sensation than a slight pricking or itching, and some tension in the part. After a short continuance in this state, small tubercles are frequently observed to arise on the inflamed surface; the skin almost always becomes dry and scaly, forming rough patches, which are excoriated and divided by furrows and rhagades. Desquamation gradually takes place; which, though it leaves behind a shining, unhealthy surface in the parts affected, yet in the first year of the disease, is rarely followed by a repetition of the appearances just described. Towards the close of the summer, or occasionally still earlier, the skin has resumed nearly its natural appearance; and but that the further progress of the disease is familiar to every inhabitant of the country, the patient might be led to flatter himself that the evil was gone by, and that there was no particular reason to dread its recurrence.

As the persons who are affected by the pellagra in this incipient stage, are rarely brought into an hospital, I have had few opportunities of observing the complaint thus early in its progress. From the instances I have seen however, I should consider the description just given of the local eruption, as being accurate in all material points. With this local affection are connected, even in the first period of the disease, certain general symptoms, which are important in as much as they indicate the constitutional nature of the malady. Debility of the whole body; vague and irregular pains of the trunk and limbs, but especially following the track of the spine and dorsal mus-

cles; headache, with occasional vertigo; irregular appetite, and general depression of spirits; these are the more ordinary symptoms which attend the early part of its progress. The bowels are for the most part relaxed; and usually continue so in the further course of the disease. There are no febrile symptoms; and in females the menstruation is generally continued without irregularity.

The remission which the patient obtains during the autumn and winter of the first year, is almost universally followed by a recurrence of his symptoms in the ensuing spring, under a more severe form, and with much greater disorder of the constitution. The cutaneous disorder is removed, and spreads itself more extensively; though still affecting chiefly the hands, neck, feet, and other exposed parts of the body. The skin becomes callous and deeply furrowed; and large rhagades show themselves, especially among the articulations of the fingers. From the cases I have seen of the cutaneous affection in this stage, I should speak of it as most resembling the inveterate degree of Psoriasis, or of the Lepra vulgaris, with some affinity certainly to the Ichthyosis, under which, as I have already mentioned, Alibert has classed it.* The debility is greatly increased in the second year of the complaint; frequently depriving the patient of all power of pursuing his active labours, and rendering him peculiarly susceptible of all changes of temperature. Partial sweats frequently break out without any obvious cause. All the nervous symptoms of the first year are renewed in more severe degree; there is a general tendency to cramp and spasmodic affections; the mind begins to suffer under the disorder; and the feeling of anxiety and despondence is very strongly marked. I find the symptom of *libido inexplebilis* noticed by one or two writers; but I did not learn this as the result of my own inquiries; and I am rather disposed to attribute the opinion to the credulity common upon this topic; or perhaps to a desire of associating

* As Alibert's plate gives but a very partial view of the appearances of the pellagrous eruption, I have written to the north of Italy, to endeavour to obtain some drawings which may illustrate this part of the disease. Should I succeed in procuring such drawings, I shall not fail to lay them before the Society.

the pellagra more closely with the leprosy, as described by ancient writers. The other symptoms already noticed, make progress as the heat of summer advances; and with greatest rapidity in those patients who are much exposed to the sun. As in the preceding year, they begin to decline towards the middle or end of the autumn; but the remission, as well of the local affection, as of the general disorder, is much less complete than before, and the patient continues to suffer during the winter from the debility, and other effects consequent upon the disease.

In the third year every symptom is renewed at an earlier period, and in an aggravated degree. The constitutional malady shows itself under a variety of forms; some of the symptoms having considerable analogy to those of scorbutus; all of them indicating a general cachexy of habit, and more particularly a lesion of all the voluntary functions. The debility now becomes extreme: the patient is scarcely able to support himself; and the limbs, besides their feebleness, are affected with pains, which still further impede the power of motion. The diarrhœa continues, and tends of course to augment this debility. Frequently a dysenteric state of the bowels comes on in the latter stages of the disease. The breath is generally fetid; and the odour of the matter perspired often extremely offensive. The appetite and digestion are irregular, yet on the whole, perhaps less affected than most of the other functions. Dropsical effusions now frequently come on; occasionally ascites, but more commonly anasarca. Vertigo, *tinnitus aurium*, and double vision are almost universally concomitants of this stage of the disorder, and all the senses become exceedingly impaired. Some spasmodic affections are very general, and these not unfrequently take a very decided epileptic form.

Connected with these latter symptoms is the effect which the pellagra produces upon the minds of those suffering under the malady, which effect forms one of the most striking circumstances in the history of the disease. The anxiety, watchfulness, and moral depression of the patient are rapidly augmented. In the hospitals appropriated to the reception of such cases, the *Pellagrosi* afford a melancholy spectacle of physical and moral suffering, such as I have rarely had occasion to wit-

ness elsewhere. These unhappy objects seem under the influence of an invincible despondency; they seek to be alone; scarcely answer the questions put to them; and often shed tears without any obvious cause. Their faculties and senses become alike impaired; and the progress of the disease, where it does not carry them off from debility and exhaustion of the vital powers, generally leaves them incurable ideots, or produces occasionally maniacal affections, which terminate eventually in the same state. As a striking proof of this tendency of the disease, I may mention the fact, that at the time I visited the Lunatic Hospital at Milan, there were very nearly 500 patients of both sexes confined there; of which number, more than one-third were *pellagrosi;* people brought thither by the termination of their disorder, either in idiocy or mania. Even this statement gives little adequate idea of the ravages of the disease, in this mode of its termination. The public hospitals of the country are wholly incompetent to receive the vast number of persons affected with the pellagra; and the greater proportion of these unfortunate people perish in their own habitations, or linger there a wretched spectacle of fatuity and decay. Where debility, as generally happens, is the cause of death, it manifests itself in the latter stage, with the usual concomitants of colliquative diarrhœa, spasmodic affections, and coma; and produces a degree of emaciation, scarcely to be surpassed in any other disease.

The mania consequent upon pellagra is often of a very violent kind, of which I have seen some remarkable instances in the Lunatic Hospital at Milan. Where such affections occur, the progress of the disease appears to be in some degree retarded, and the strength less rapidly declines. Attempts at suicide are frequent among Pellagrosi; and particularly, it is said, by throwing themselves into the water. Strambi has thought it worth while to characterize this tendency by the name of Hydromania.

Though for the sake of brevity, I have described this train of symptoms as going on from the third year, yet I may remark that the pellagra is generally of longer duration; and that other intermissions usually occur in its progress, giving the patient a certain relief in the degree of his sufferings, though

little hope as to the event of the disease. In some instances the cutaneous affection forms the principal indication of the complaint for several successive years; being renewed every spring, and disappearing again in the autumn. In other cases, where it has been found possible to remove the patient to a new situation and mode of life, the disease is still further arrested in its progress. It rarely happens, however, that these means can be practically adopted; and the constitutional malady is generally so far established in the third or fourth year, that little hope remains of benefiting the patient, either by medicine, or change in the mode of life.

I cannot learn that morbid dissections have thrown any light on the pathology of this disease. In some patients the liver, in others the spleen, have been found enlarged and indurated: marks of disease are occasionally seen in the intestines and mesenteric glands, but these appearances are by no means constant; and may more reasonably be considered as effects, than as causes of the malady.

I have already stated the fact, that the pellagra is almost exclusively confined to the lower classes, and chiefly to the peasants who cultivate the soil; the disease being rarely seen in the cities of Lombardy, except in the instance of patients who have been brought thither from the country. I may now add to these facts, that persons of each sex and of every age, are liable to be affected by it; and that the hereditary tendency of the disorder is proved by the circumstance of the symptoms frequently appearing in infants at the earliest period of life. This fact is particularly noticed to me by my friend Dr. Sacco of Milan, who in his frequent journies through the country, as Director of the Vaccine Establishment in this part of Italy, has had the best opportunities of observation on the subject. The authors who have written on pellagra, as far as I have had the opportunity of seeing them, all concur as to this point, and cite the numerous instances in which the disease continues in families, with an evident predisposition derived from parents who have suffered under it. The symptoms of the disorder, as it appears in infants, are not materially different from those attending it in more advanced life. The cutaneous affection of the hands, arms, and feet, is that which first shows itself; re-

newed and augmented in successive years, and attended with the various symptoms which indicate a cachectic state of the body, and which in the greater number of instances have a fatal termination.

The question whether the pellagra be contagious or otherwise, has of course attracted the attention of all, who have had opportunities of observing the disease. This point, as I believe, on the best authority, has been decidedly settled in the negative. Setting aside particular instances, the general facts may be mentioned, that the disease is almost exclusively confined to the lower classes; that of people living together in the same habitation, some have the disorder, others not; and that in the case of married people, it frequently happens that the husband or the wife is pellagrous, while the other remains wholly unaffected.

From the preceding history of pellagra, in its ordinary form of occurrence, it will be seen that the disease has in some points a certain degree of affinity to the leprosy of the middle and early ages; and this relation has been noticed by most of the authors who have written upon it. The differences, however, in other points, are so distinctly marked, as not to admit of the diseases being classified under the same name; and this remark may be further applied to the scurvy, with which the pellagra is connected by several common symptoms. By some authors indeed, it has been described under the name of the scorbutus alpinus; but the greater number have considered it (and apparently with more reason) as a separate disease; though occasionally complicated with the genuine scurvy, which prevails as a frequent disorder in the same districts of country. Titius, one of the writers upon the pellagra, has defined it *Erysipelas, periodicum, nervosum, chronicum;* taking as the basis of this definition, the appearance of the eruption as it first occurs on the skin.* Its after progress, however, and the concomitant constitutional symptoms, are so different from those of erysipelas, in the common understanding of the term, that it would be difficult to admit such a definition, as giving the nosological arrangement of the disease.

* Salom. Constant. Titii Oratio de Pellagræ Pathologia. Viteberg, 1792.

With regard to the origin and causes of this singular endemic malady, though a good deal has been written both by Italian and German physicians, yet I do not find any thing which can be regarded as perfectly decisive or satisfactory. The antiquity of the pellagra has been, in particular, a subject of doubt and controversy. Moscati, a Milanese of considerable eminence in science, has stated it as his opinion, that the disease has not been known in Lombardy more than fifty years; and other writers have held the same belief on the subject. A greater antiquity is assigned to it by Strambi, who, being appointed by Joseph II. the director of a hospital established at Legnano, near Milan, for the reception of pellagrosi, had the best opportunities of making himself acquainted with the disease. In his Treatises on the Pellagra, published in three successive years, from 1784 to 1787,* he mentions the fact of his having seen many pellagrosi in the hospital, who gave him distinct assurances of their fathers and grandfathers having had the disorder; and from some particular instances, he thinks himself entitled to believe that the pellagra must have been known in Lombardy at least sixty or seventy years before the time when he wrote. Francisco Frapolli, physician to the hospital of Milan, who published a short description of the disease in 1771†, contends also for its antiquity; and supposes it possible that it may be the same disorder as one called *Pellarella*, which is casually noticed among the records of the Milan Hospital for the year 1578. This supposition, however, stands without any other proof than the bare analogy of name; and on the whole there is sufficient ground for concluding, from the silence of all preceding Italian medical writers as to this disease, that it cannot have existed under any well-defined form for more than a century, perhaps not even for so long a period.

However this may be, it is certain, from the concurrent observation of all who have attended to the subject, that the pellagra has been rapidly spreading itself during the last fifty

* De Pellagra Observationes, quas in Regio Pellagrosorum Nosocomio collegit Caietanus Strambio, Regius ejusdem Director. 1784-87. Mediol.

† Francisci Frapolli, Mediolanensis Nosocomii majoris Medici Animadversiones in Morbum vulgò Pellagram. Mediol. 1771.

years; and with greatest rapidity during the latter part of this period. At the time when Strambi wrote, in 1784, he calculated that the pellagrosi formed about a twentieth part of the population in the districts principally suffering under the disorder. It now appears that there are districts in the Milanese territory where the proportion of pellagrosi is one out of five or six in the whole population; and from the intercourse I have had with the medical men in this part of Italy, I am led to believe that the evil is augmenting at this moment in a very alarming degree. I may repeat here the fact I before stated, that the districts thus particularly affected are in the Alto-Milanese, where the country rises towards the Alps, which form its northern boundary. Soler, one of the writers who have treated of the pellagra,* has proposed a division of the disease into the sicca and humida, founded upon some difference of symptoms, which he believes to exist in the disorder, according as it appears in dry and elevated situations, or on the flat and moist surface of the plains. As I do not find, however, that this division or difference of symptoms is recognized by other authors, it is sufficient to mention the opinion, without entering into further details respecting it. At the same time it seems certain that the disease prevails in some districts much more than in others; that it appeared first in the higher parts of the Milanese territory, and that in this tract of country its ravages are still more extensive than in any other part of Lombardy.

The question regarding the causes of the pellagra I have already alluded to, as involved in much obscurity; and though the current opinions on the subject are certainly on the whole the most plausible, yet they must still be admitted to want something to render them satisfactory. In reference to the remote causes, the most important facts in evidence undoubtedly are, the limited period during which the disease appears to have existed in the country; its being confined almost exclusively to the lower classes, and its rare appearance in the towns or cities of Lombardy. These several circumstances

* Osservazioni medico-pratiche sulla Pellagra, del Luigi Soler. Venezia, 1791.

may be admitted to prove, that the disease depends upon some present peculiarities in the mode of life of the peasants of this country. The climate is obviously not the cause concerned; since this, as far as it is known, has been unchanged for a much longer period, than that which includes the history of the pellagra; or had it been changed, would have affected alike both the higher and lower classes of the population. The same objections may be made to the opinion that any circumstances of mere locality are concerned in producing the disease. It may possibly be true that the plains of Lombardy are more frequently and irregularly flooded than formerly, and that the general surface is more marshy and unwholesome; but this does little to explain the causes of a disorder, which is chiefly prevalent in the higher lands, where such changes have not equally taken place.

The point then to which we are almost necessarily conducted, is the mode of life and subsistence among the peasantry of the country. All those with whom I have conversed on the subject, and almost all the writers on the pellagra, concur in assigning this as the principal cause of the disease. Though I have spoken of Lombardy as one of the most fertile portions of Europe, yet to those who consider the little certain relation between mere productiveness of soil, and the prosperity or comforts of the population dwelling upon it; it will not appear very extraordinary that the peasants of this district should be subject to various physical privations, unknown to the people of countries which are much less favoured by nature. The fact unquestionably is, whatever our speculations as to the cause, that the peasants of Lombardy do for the most part live in much wretchedness, both as regards the quantity and quality of their diet, and the other various comforts of life. It further seems probable, if not certain, that this evil has been progressively augmenting within the last fifty years; partly, perhaps, an effect of the wars which have so often devastated the country by marches and military contributions; partly a consequence of the frequent changes of political state; together with the insecurity, the variable systems of government, and the heavy taxes and imposts attending such changes. To these causes may be added, a decaying state of commerce, and a faulty

system of arrangement between the landlords and the cultivators of the soil; all tending to depress agriculture, and to reduce the peasantry at large to a state of much misery and privation.

The ordinary diet of these people consists chiefly of maize, prepared in various ways; of rice, millet, beans, and some other articles of vegetable food. Their bread, which is principally made from maize, is for the most part of bad quality; ill fermented, and not unfrequently deficient in salt. Animal food rarely forms a part of their diet; and though living on a soil which produces wine, their poverty almost precludes the use of it, even when sickness and debility render it most needful. The same condition of poverty is evident in their clothing, in their habitations, and in the want of all the minor necessities and comforts of life. The immediate effect of these privations is obvious in the aspect of squalid wretchedness and emaciation, which forms so striking a spectacle at the present time throughout the greater part of Lombardy. I say particularly *at the present time;* because whatever may have been the progress of misery among the peasants of this country during the last half century, it appears to have increased in a tenfold ratio, during the last two years; the effect of bad harvests added to the preceding wars and political changes which have distressed this part of Italy.

This general condition of the peasants of Lombardy seems to be the cause to which we may most reasonably attribute the prevalence of the pellagra amongst them. Their bad and deficient diet may be presumed to produce a cachectic habit of body; manifesting itself progressively in the cutaneous eruption, in muscular debility, and in a general lesion of the nervous system; including the senses, and the mental faculties. I am aware, that I am here using very general phrases; but it will be readily admitted, I believe, that the state of our knowledge as to the causes of disease, does not often admit of others more definite; and though I might cite various speculations which the writers on pellagra have adopted, founded chiefly on the doctrines of the humoral pathology, yet I refrain from this, under the conviction that they have added little or nothing to our real knowledge of the subject. The general fact

that a deficiency of good nourishment and other means of comfortable subsistence, is capable of producing constitutional disease, is attested by numerous analogous instances. The scurvy may be mentioned as one closely allied in several respects, though differing from the pellagra, as well in its symptoms, as in the particular nature of the diet, which appears to have effect in producing the disorder. Other forms of disease, more closely akin to the pellagra, I have observed in Iceland; connected apparently with the diet and mode of life of the inhabitants of that island. Some of these I have described in a Thesis on the diseases prevailing among the Icelanders, published in 1811. Here too it may be remarked, that the peculiarity of diet is of different kind; and that the points of similarity of condition are chiefly such as relate to general privation and want of cleanliness; causes which appear to have more effect in producing the diseases of this class, scurvy perhaps not excepted, than any of those peculiarities of animal or vegetable food, which are usually alleged and insisted upon by medical authors.

The pellagrous habit of body, however produced among the peasants of Lombardy, is certainly continued and extended by the hereditary tendency of the disease. I have already mentioned the fact of its occasionally appearing in infants at the breast; and though this, perhaps, is comparatively rare, yet it seldom happens that the children of pellagrosi escape the disorder as they proceed in life, and are exposed to what appear to be the exciting causes of the complaint. The early occurrence of the Pellagra, if the disease depends upon bad and deficient diet, might indeed very reasonably be expected. The mother, sickly and emaciated from poverty, is often unable to nurse her offspring; and the substitute for her milk is generally a pulse, prepared from maize and water; the effect of which is very obvious, in disordering the bowels of the infant, impeding nutrition, and preparing the way for future disease. The same circumstances of defective nourishment are continued during childhood, and with similar effect upon the constitution.

Assuming, however, the physical condition of the peasants of Lombardy as the most probable cause of the prevalence of

the pellagra in this district, it may still be asked, why the disease does not equally appear in other countries, where the state of the population is not less miserable? Though there be some difficulty in answering this question, yet on the whole it will probably not be thought inconsistent with medical experience, to suppose that one form of hereditary disease may be accidentally produced in a particular district, and afterwards continued and extended under the influence of the same general causes, which produce other, though analogous maladies, elsewhere. Or if this reasoning be thought vague or improbable, it may perhaps be better to plead ignorance of the local peculiarity; still not relinquishing the opinion before giving as to the general causes of the disease, which can scarcely be doubted, considering the evidence derived from the different circumstances already described.

I may mention here, that several Italian physicians have conceived the increasing use of maize as an article of food, to be much concerned with the extension of the pellagra in this part of Italy, and have thought it probable that the peculiar nature of the disorder might be derived in a great degree from this particular diet. It is a strong objection to this opinion, that the disease, as far as I know, is not observed in other parts of the south of Europe, where the same diet is as generally in use among the lower classes. I have never seen it in the northern parts of Greece, though here the same grain forms a principal article of food with the peasantry of the country; and though I shall afterwards notice a disease occurring in the Asturias, which has some resemblance to the pellagra, yet with this exception, I do not know of its existence in Spain or Portugal, where the maize is very extensively used. The same objection may be made to the idea suggested by some physicians, that the increasing use of rice in Lombardy has been a principal cause of the disorder. It is true that quotations might be obtained from different medical writers, stating the noxious effects of warm rice, as an article of food, in producing nervous affections, blindness, &c.; but even were we to admit much of this, there would still remain a wide disproportion between the alleged cause, and the extent of the effects, as they appear in the pellagra of Lombardy. It may

further be noticed that the use of rice was common in Italy in former times, as we find from the testimony of Horace and Pliny, without the production, as far as we know, of any malady corresponding to that now under consideration. If the cultivation of rice in Lombardy has any connexion with the pellagra, it is probably much less from its effects as an article of food, than from the swampy nature of the districts in which it is most successfully grown. In allusion to this circumstance, however, I have already noticed the fact, which seems to be established, that the pellagra is most frequent in some of the higher parts of Lombardy; and though the unhealthy influence of a marshy surface in this climate, is well understood from experience, yet it would seem that the diseases depending specifically on this cause, are very different in character, from the one I have just been describing.

On the whole, it is certainly to the leprosy or elephantiasis of the middle or dark ages, that we are to look for the nearest resemblance to the pellagra, both in the character and probable causes of the disease. Whatever be the doubt as to the correct application of these terms, and however various the disorders that were admitted into lazarettoes, it is clear that certain diseases were very prevalent in Europe at that period, attended with scaly or tubercular eruptions, or occasionally with both; and bringing on great debility, defect of the senses, and other constitutional symptoms. It appears equally probable that these diseases had their origin, and were continued, by the condition of the European nations at the time in question; when bad governments, imperfect agriculture, and numerous internal wars, had the effect of producing occasional famine and much habitual distress among the lower classes. It is true that we do not find the description of any of these forms of disease exactly corresponding with the pellagra; but the analogy is nevertheless sufficiently close, to warrant us in presuming the same general causes, where the circumstances and effects are so far alike.

I must not quit the subject of the causes of the pellagra, without alluding again to a remarkable fact in the history of the disease; viz. the first appearance of the symptoms in the spring of the year; their partial disappearance in the autumn;

their renewal in the ensuing spring; and the continuance of this alternation for successive years, whenever the disease is protracted thus long, without reaching its latter stages. The generality of this fact, and the further circumstance of the eruption chiefly appearing on those parts of the body which are exposed to the sun, have led some physicians to suppose that *insolation* was the actual cause of the disease. Albera, one of the writers on the pellagra, describes it in the title of his work, as *malattia dell insolato di primavera, volgarmente detta la Pellagra.* Frapolli, after stating various arguments in support of this opinion, asks the question "*Nonne satis evidens et unica causa insolatio?*" and proceeds to give a laborious hypothesis, illustrating its mode of action upon the body. It is obvious from his reasoning on the subject, that he does not sufficiently distinguish between remote and exciting causes. It may be admitted as very probable that insolation is one of the exciting causes of the Pellagra, and that in part at least it determines the appearance and violence of the disease during the spring and summer. But it is certain that exposure to the sun cannot produce the disease, or bring it to its termination, when produced; the obvious proof of which is that the same, or a greater exposure, is incurred by the peasants in every part of Italy, without any similar effects resulting from it; to say nothing of the still more extensive negative evidence to this effect derived from other countries. It may further be presumed that insolation is by no means the sole exciting cause, which determines the appearance of the symptoms in the spring. It seems probable that something of this effect is owing to the increased labours of the peasantry at this season; which cause, though it seems slight and ordinary, yet may reasonably be supposed to have some influence, where the disease is already in the habit, and prepared to manifest itself. Perhaps also it might be surmised, that the periodical returns of the pellagra, during its early stages, depend in part on the natural periodical changes of the body itself, independently of the external causes just alluded to; but this is a topic too extensive, as well as too obscure, to admit of my entering upon it here*.

* It may be noticed that Alibert describes a variety of the disease, which

With respect to the medical treatment of the Pellagra, I have much less to say than might perhaps be expected from the preceding history of the disease. The truth is, that in a malady, thus extensively prevalent among the lower classes, and depending chiefly, as it would seem, on their diet and mode of life, no ordinary methods of cure can be adopted with a reasonable prospect of success; the evil requiring those more general preventive means, which it is out of the power of medicine to afford. By far the greater proportion of patients affected with Pellagra are unable to obtain admission into the hospitals of the country; the number and finances of which are wholly inadequate to the demands from this source. Their poverty equally precludes medical assistance in their own habitations, or the changes in diet, &c. which are essential to their relief. Those who obtain admission into the hospitals, generally come there after the disease has already made much progress in the constitution; and even in the cases where benefit has been derived from this change in the mode of life, it is speedily lost again by the necessity of returning to their own houses and former occupations. The only considerable establishment, as I know, set on foot expressly for the reception of *Pellagrosi*, was that, before alluded to, at Legnano, in the Milanese territory. It was abolished, however, five years after its institution, by the Emperor Joseph II.; and in lieu of it, a provision was made for receiving a certain number of *Pellagrosi* into each one of the principal hospitals throughout the duchy of Milan; the funds of the former establishment being applied to this object. Only sixty patients could be received into the hospital at Legnano. The present plan of distribution supports a greater number; but nevertheless appears to have done little in mitigating the evil, either by cures actually performed, or by suggesting any more extensive and radical means of obviating the disease. In 1786, the Patriotic Society of Milan proposed a premium for the best treatise on

he calls *Ichthyosis, Pellagra orbicularis*, from the desquamation taking place in circular patches; in which the pellagrous eruption does not appear to take place precisely on parts of the body exposed to the sun, and occurs indifferently at any time of the year. It is very likely that such a variety may exist, though it does not happen that I have heard it described.

the history, prevention and treatment of the Pellagra; but equally without obtaining any results of the least practical value. It is not a light task to remove causes which affect a whole community of people; and rarely can such object be accomplishing by any sudden or artificial means; even though governments engage themselves in pursuing it.

The actual medical treatment of the cases of Pellagra brought into the hospital, may be very briefly described. A nutritious diet is immediately adopted for the patient, unless contra-indicated by some local affection, and wine and tonic medicines are given with the same view. A decoction of the lichen islandicus is in common use in the hospital of Milan, as a part of this plan of diet. The warm bath is generally employed at the same time; a favourite remedy among the peasantry afflicted with the Pellagra, probably in consequence of its comparative cheapness; but esteemed also by most of the medical practitioners, who are concerned in treating the disease. Diaphoretics, and especially antimonials, are in considerable use; under the idea of correcting the state of the skin, and getting rid of the morbid humours through this channel. Depletion by blood-letting is very rarely employed, except where some local inflammation happens to occur, or in the mania which sometimes supervenes upon the disease. Attention is of course paid to the bowels; in correcting the diarrhœa, and bringing them as far as possible to a natural state. Anti-scorbutic medicines are occasionally used; but I did not find reason to believe, from the inquiries I made, that they were of much avail. In short, it appears certain, that mere medicine has done very little for the relief of Pellagra; and Strambi, whom I have before mentioned as the inspector of the hospital at Legnano, frankly confesses that he never saw a case distinctly cured by the remedies that were employed. It is true that Frapolli speaks of numerous cures that have been obtained by the conjoint use of the warm bath, frictions, and sudorific medicines; but this assertion does not seem to have acquired much credit with succeeding physicians; and the general acknowledgment is one of inability and failure.

In the course of this paper, I have already noticed several authors who have written upon the Pellagra during the last

half century. To these I may now add the names of Gherardini, Zanetti, Odoardi, Fanzago, and Jansen; the titles of whose respective treatises are given in the subjoined note*. Those of Fanzago, the professor of medicine at Padua, published successively in 1789 and 1792, may perhaps be considered the most valuable documents as to the history of the Pellagra. I have not had the opportunity of consulting these works since I left Italy; but should I be enabled to procure these thence, which I have some expectation of doing, I shall have the honour to present them to the library of the Society.

Before concluding this paper, I may notice briefly the existence of a disease in the Asturias, which has been described by Thiery†; and which, from its analogy to the Pellagra in many of its symptoms, has been alluded to by several of the writers on the latter disease. The description of Thiery, however, would make it appear, that this malady much more closely resembles the Lepra Arabum or elephantiasis; the general relation of which to the Pellagra, I have already had occasion to point out. I am not aware of the peculiar circumstances which produce or maintain this disease in the Asturias; but it is not unlikely that they resemble those which have been already mentioned as the probable causes of the Pellagra of Lombardy; and which appear capable of producing several different forms of constitutional disease, attended with cutaneous eruptions.

* Della Pellagra Descrizione di Michelle Gherardini. Milano, 1780.

Francisci Zanetti, de morbo, vulgo Pellagra, dissertatio. Nov. Act. Nat. Curios. Tom. VI. Norimb. 1778.

D'una specie particolare di Scorbuto; dissertazione di Jacop. Odoardi; Nuova Raccolta d'Opuscoli Scient. &c. Vinezia, 1776.

Memoria sopra la Pellagra del Territorio Padovano, da Francisco Fanzago. Padova, 1789.

Paralleli fra la Pellagra ed alcune malattie, che più lo rassomigliano, del F. Fanzago. Padova, 1792.

W. X Jansen de Pellagra, morbo in Mediolanensi Ducatu Endemio. Lugduni. 1787.

† Observations de Physique et de Médicine, faites en differens Lieux d'Espagne. Par M. Thiery. Paris, 1791.

Account of a Large Wen, successfully extirpated

BY JOHN SYNG DORSEY, M. D.

[From the Transactions of the American Philosophical Society, Vol. 1. New Series.]

THE paper which I have the honour of presenting to the Society, contains the history of a steatomatous tumour, of very unusual magnitude, successfully extirpated.

The patient, Julia Richards, a negro woman, from Carlisle, in Pennsylvania, was aged about forty-five years, and enjoyed good health; she was corpulent, but active, until her exertions were restrained by the incumbrance of her tumour.

She stated that it had been first noticed about eighteen years before I saw her;—that it had grown gradually, and had never been painful. When she applied to me, her attitude in walking resembled that of a woman carrying a large and heavy sack. On examination, I found the tumour arising at the upper part of the back, extending equally on both sides; and although pendulous from its weight, yet the root of it was very large. The dimensions were as follow:

Circumference at the neck or narrowest part of the tumour, two feet ten inches.

Circumference at the thickest part, vertically, three feet nine inches.

Circumference horizontally, three feet one inch and a half.

The circumference of the waist, after the wen was removed, was two feet nine and a half inches; so that the narrowest part of the tumour was thicker than the patient's body.

The surface of the tumour was tolerably regular, but very large and numerous veins were seen in various parts of it.

The patient was admitted into the Pennsylvania Hospital, and on the 22d of February, 1815, I proceeded to remove the tumour. Having previously administered an opiate, I placed her, at the suggestion of Dr. Physick, on her face upon the table, fifteen minutes before commencing the operation, and directed assistants to elevate the tumour in such a manner as to empty it as completely as possible of blood; and

I was greatly delighted to perceive the change in the size of the superficial veins, which resulted from this simple expedient; many of them contracted and could not be perceived.

The operation was commenced by external incisions, calculated to preserve skin enough to cover the surface left by the removal of the tumour; and this skin being dissected and turned back, which was the most tedious part of the operation, the tumour, by large and rapid incisions, was detached from its base and removed. It adhered to some of the spinous processes of the vertebræ, and to the muscles and tendons near the spine. The operation occupied twenty-one minutes; and the loss of blood was very trifling.—The skin was found to adapt itself very well to the denuded parts, and was secured by strips of adhesive plaster, compresses and bandages.

The greater part of the sore united by the first intention; no unpleasant symptoms occurred, and the patient was discharged cured, on the 15th April. She is at this time, and has been ever since the operation, perfectly well.

The tumour was found to weigh twenty-five pounds, but when filled with blood, was probably much heavier.

The tumour of Eleanor Fitzgerald, described by Mr. John Bell; and that of a negro woman, published in the Medical Repository of New York, (Vol. III. New Series) were of enormous magnitude but adherent by small bases. The basis in the present instance was very great, and I am not aware that so large a tumour has been ever before extirpated.

Remarks.—The most important practical precept derived from this case, is the influence of position on the circulation of the blood. I once attended an operation on a tumour of comparatively small size, seated on the back, the extirpation of which was found impracticable, in consequence of bleeding from the superficial veins. In the treatment of hemorrhagy from blood-vessels in the extremities, and on certain local inflammations, an elevated position is often found of great importance. I have seen a bleeding from an artery in an aneurismal arm, in which circumstances precluded the use of a ligature or tourniquet, effectually arrested by an elevated posture, the hand being constantly kept in a vertical position.

These remarks, although somewhat digressive, are in my opinion of too much importance to be omitted. The practice of employing position to empty blood-vessels for surgical purposes, in the case alluded to; and others, so far as I know, originated with Dr. Physick; and my own experience has afforded numerous proofs of its value, and convinced me that it has been too much neglected by surgeons.

Analysis of the Blue Iron Earth of New Jersey.

BY THOMAS COOPER, M. D.

[From the Transactions of the American Philosophical Society, Vol. 1. New Series.]

THIS earth is found in many places of New Jersey, not usually of a blue colour when dug up, but acquiring that colour after being exposed to the air. I do not enter into any geological details, because I have reason to believe this part of the subject will occupy the attention of a member of this society, better qualified, from local knowledge, to treat it than I am. I offer to the Society the following experiments, because I think the nature of the substance has been mistaken.

External Characters.

1st. It has a moderately deep smalt-blue colour, neither verging toward copper-red, like indigo and Prussian blue; nor toward green, like mountain blue, and blue fluor.

2d. Its appearance to the eye is earthy.

3d. When breathed upon, it gives out a slight earthy odour.

4th. It is moderately hard: when scratched by the point or edge of a common penknife, the streak is bluish and dusty, from the knife penetrating the substance of the stone; but when scratched by the back of the knife, or by the nail, the streak is greyish-white and shining.

5th. It breaks with a fracture somewhat conchoidal.

6th. It adheres to the tongue with some force.

7th. When a drop of oil is poured on it, the colour becomes

black, or of a deep blue, approaching to black: so that it promises to be useful as a pigment.

8th. Its specific gravity is 2,5338.

9th. It absorbs 35 per cent. of water on immersion in that fluid.

Its Habitudes when exposed to Heat.

10th. Before the blow-pipe, whether supported by charcoal, or on the bottom of a crucible, it becomes of a brown colour, and then melts into a shining greyish globule, not attractable by the magnet, unless this treatment with charcoal be continued.

11th. When this stone in powder is distilled at a low red heat for an hour in close vessels, with a pneumatic apparatus, no gas is collected, except the common air expelled by rarefaction from the containing vessel; moisture arises, which by means of a cold atmosphere, is condensed into pure water, exhibiting no chemical qualities on dropping a drop of it into muriat of baryta, or diluted tincture of galls. The powder thus distilled, loses from 23 to 24 per cent. when the distillation has been urged.

12th. On calcinating 100 grains in a full red heat for an hour, 80 to 81 grains of a bright brown powder are procured, consisting (as will be seen) partly of the brown-red oxyd of iron, and partly of alumina.

13th. This calcined brown powder, when treated in a crucible with charcoal, or when made into a paste with wax, and burnt three several times, yielded 48 grains of iron, by means of a magnet; and there were left behind, six and a half grains of a powder not acted upon by the magnet. This last, being dissolved in sulphuric acid, diluted, and precipitated by carbonate of ammonia, possessed the common characters of alumina.

14th. When fused in small proportion with potash, for the purpose of discovering the presence of manganese, the trace of green colour was too slight to indicate any appreciable quantity of that substance.

Its Habitudes with Solvents.

15th. This stone does not dissolve perceptibly in pure rain

water at the common temperature of the atmosphere; nor does the water in which it has been infused, exhibit any colour when tested by galls, or triple prussiat of potass: nor does it afford any precipitate by carbonat of potass.

16th. The sulphuric, the nitric, and the muriatic acids, dropt on this stone, excite no effervescence; they are diffused on it, and sink into it.

17th. It is not easily dissolved in *sulphuric* acid, without the aid of moderate heat. I used the top of the common tenplate stove, of about 150° of Fahrenheit; but with heat, and after some hours digestion, it becomes a white mass with this acid; which mass is soluble entirely in boiling water, producing a clear solution; there is no effervescence during the solution.

18th. Having dissolved 50 grains of the stone in sulphuric acid, and diluted the solution with hot water, I added a filtered solution of triple prussiat of potass, made in the common way, by digesting carbonat of potass on Prussian blue, till the latter no longer became discoloured. On continuing to add this while any precipitate appeared, I obtained a quantity of the most intense and beautiful Prussian blue, which, when calcined in a full red heat for an hour, yielded forty grains of red oxyd. The remaining liquor, after filtration, and precipitation by carbonat of ammonia, afforded a precipitate which, when carefully filtered, and moderately dried, weighed about 4 1-2 grains: it was alumina. It weighed less in proportion than the alumina of experiment 13, in consequence of not being so much dried. The quantity thus obtained, of iron and alumina, is probably not quite accurate, owing to the iron contained the triple prussiat: but very near the truth.

19th. This stone when powdered, dissolves in *nitric* acid in the warm atmosphere of a summer's day, after 10 or 12 hours digestion, without residuum.

20th. This nitric solution was evaporated to perfect dryness; the powdered residuum was digested in fresh nitric acid, which was again evaporated to dryness. A third portion of the same acid was now poured on the brown-red powder, and digested on it: about 1-10th was dissolved, as appeared on drying and weighing the residuum. The solution, moderately

diluted and filtered, exhibited but very slight shades of colour with tincture of galls and solution of triple prussiat of potass, so that but a trace of iron was taken up, as was originally presumed and intended: the digestion in nitric acid, and driving it off by heat, being meant for the purpose of oxyding the iron beyond the point of solubility in that acid. The last portion of nitric acid, therefore, took up nothing but the earths. These being precipitated by carbonat of ammonia, afforded about nine per cent. of earth, when dried at the heat of 150 Fahrenheit, and consisted entirely of alumina.

21st. *Muriatic* acid dissolves this stone by heat. The solution is of a brownish-yellow colour. It leaves no residuum.

22d. *Oxalic acid* and oxalat of ammonia, occasion no precipitate from any of these solutions: hence there is no trace of any of the alkaline earths.

Experiments to discover the Presence of Prussic Acid in this Stone.

23d. Carbonat of potass digested on the powdered stone, takes away the colour, but does not dissolve the substance. The solution of carbonat of potass so digested on the stone in fine powder, being filtered, produced no blue precipitate when poured into a solution of sulphat of iron.

24th. A piece of this stone, suspended for many hours in a solution of sulphat of iron, diffused no trace of blue colour, either when immersed dry, or moistened with an alkaline solution.

25th. The following experiment was suggested and made for me by Mr. Cloud, who appears to have investigated the properties of palladium more fully than any other chemist. The nitro-muriat of palladium, and the nitro-muriat of gold, are not precipitated by the chromat of potass, but they are precipitated by the prussiats. When the red oxyd of mercury is triturated with Prussian blue, and boiled with water for half an hour, a prussiat of mercury is formed, which occasions a fawn-coloured precipitate, when added even in a very minute portion to the nitro-muriat of palladium: this precipitate is a prussiat of palladium. Red oxyd of mercury was mixed with

the blue earth in fine powder, and water being added to the mixture, was boiled in a sand-bath for more than half an hour, and constantly stirred during the time: when cool, the liquor was filtered and dropped into a nitro-muriatic solution of palladium, but no precipitate appeared.

Experiments to ascertain the presence of Phosphoric Acid.

PRELIMINARY OBSERVATIONS.

When the native green phosphat of lead is melted before the flame of the blow-pipe on charcoal, it crystallizes, on cooling, into a polyhedral garnet-like kind of crystallized mass; so does the artificial phosphat of lead, made by adding phosphat of soda to nitrat or acetat of lead; in which case the phosphat of lead precipitates in a dense white powder, speedily and distinctly. These remarks have been made by Klaproth, in his Analysis of the Phosphated Lead Ores.

When nitrat of lead, or acetat of lead, are added to any solution containing phosphoric acid, the solution of lead is instantly decomposed, and a phosphat of lead is formed. Thus, when phosphat of iron, made either directly by solution in phosphoric acid, or by precipitation by phosphat of soda, is dissolved even in small quantity in the nitric acid, this solution is immediately precipitated by any solution of lead: these facts were previously ascertained.

Moreover, solutions of iron in phosphoric acid, were made both directly and by double decomposition, as by precipitating a solution of sulphat of iron by phosphat of soda. The phosphat of iron in both cases, when dried moderately, assumes a slight bluish tinge by exposure to the atmosphere: which may have led to the supposition of this blue stone being phosphat of iron. These previous experiments were made to ascertain the colour of artificial phosphat of iron.

With these facts in view, a solution of the blue iron earth, or stone, was made in pure nitric acid, freed from muriatic acid by nitrat of silver, and from sulphuric acid by nitrat of baryta: precautions which were afterwards found unnecessary for this particular purpose, the common nitric acid of commerce answering sufficiently well. This nitrated solution of the substance under analysis, was mixed gradually with nitrat

of lead, and subsequently in a distinct experiment with acetat of lead. In neither case was there any precipitate produced, as might have been expected to take place, had even a trace of phosphoric acid, combined or uncombined, existed in this nitric solution.

Again, a considerable quantity of the substance in powder, dried, but not discoloured, was rubbed up with about 1-10th of its weight of lamp-black: in another experiment with 1-10th of sulphur; in a third experiment with 1-10th of a mixture of lamp-black and sulphur. The mixed powder was put into bottle-shaped crucibles, having clay stoppers, with a glass tube of about 1-16th of an inch diameter, passing through the stopper. The clay was burned to fit the aperture, and during the experiments the stoppers were also well luted and attended to.—The mixtures were exposed to a gradual heat for half an hour, to dissipate the hygrometrical moisture, if any should remain; the stoppers constantly examined: the heat was gradually increased to a full red, at the close of an hour: during this time a lighted paper was very frequently applied to the orifice of the glass tubes whence the vapours from the blue earth issued, but there was no trace of any thing inflammable to be discovered. Nor was any, the slightest phosphorescence discovered, on dropping the powdered stone on red hot charcoal. Hence I conclude,

1st. That the hard blue earth of New Jersey is probably the same substance with the blue earth of Jamison and Werner, the fer azurè of Hauy, and the fer phosphatè of Brochant and Brogniart; but it seems to differ somewhat from the smalt-blue fossil of the Vorau, analysed by Klaproth.

2d. That this blue earth of New Jersey, contains neither prussic or phosphoric acid.

3d. That it consists of sub-oxyd of iron, intimately united with about 1-10th of the earth of alum, and 24 per cent. of water, probably in chemical union.

4th. That it contains no perceptible quantity of silica, lime, magnesia, or the other earths.

5th. That its colour may be of vegetable origin, but I cannot venture any probable surmise concerning it.

The crystallized earth of New Jersey, consisting of olive

green crystals, upon a bluish green earthy stone, is very similar in its geological and chemical characters, to the blue earth just described; but as I propose a more perfect analysis of these crystals than I have yet made, I shall say no more about them at present.

THOMAS COOPER.

Observations on the treatment of the Venereal Disease, without Mercury. By G. J. GUTHRIE, ESQ. *Deputy inspector of Military Hospitals, Surgeon to the Royal Westminster Infirmary for diseases of the Eye, Lecturer on Surgery, &c.*

[From the Medico-Chirurgical Transactions, Vol. VIII. for 1817.]

THERE are no diseases to which the male sex is so very obnoxious as those of the sexual organs, and there are none which have more occupied the attention of surgeons; yet there is not a subject in surgery of equal importance, on which less has been written since the time of Mr. Hunter. We find that those who have had the greatest opportunities of acquiring knowledge, have for the most part refrained from communicating to the public the results of their observations; and that this has arisen rather from the difficulty of the subject than from its being so throughly understood as to require no comment, will be immediately acknowledged by every one of discernment and experience. In offering a few observations on the treatment of diseases acquired through promiscuous intercourse, I wish I could think they would elucidate a subject beset with so many difficulties; but the more I consider in which way this may be accomplished, the greater I find the obstacles to be surmounted, except I could at once adopt the opinions of a French anonymous, but very ingenious author*, "that there is not, nor even was such a disease;" but to this opinion there are equally insurmountable facts to be

*Sur la non-existence de la Maladie Vénérienne. Paris, 1811.

opposed. Certain however we may be in this subject of intricacies, that the venereal disease has, within these few years, totally altered in many of those properties and effects, which are called specific; or that the greater part of the opinions which have been commonly entertained are erroneous.

In offering these remarks, I beg to be considered as merely giving a few slight sketches of opinions and facts which may be hereafter confirmed, filled up, or even abandoned, as circumstances and further observations may render necessary; and I shall confine myself as much as possible to facts, and enter as little into the consideration of opinions, that are well known and usually entertained.

On the continent in general, little attention is paid to the appearance of primary sores: if a man have had a suspicious connexion, followed by ulcers on the glans penis, or prepuce, or even a gonorrhœa, he is at once declared to be infected with the venereal disease; but this does not lead in general in Italy, or in the south of Europe, to the exhibition of mercury or any other specific. In France, and particularly in Paris, the contrary is the general practice: the patient is placed on the use of the oxymuriate of mercury; and after taking about thirty-two *portions*, in half doses, twice in the day, which generally occupy the same number of days, he is considered free from disease; and this will in most cases be true in all kind of sores which have originated from sexual intercourse; but if the ulcers should not heal in this period, or secondary symptoms supervene, he frequently continues the medicine for an unlimited time. In doing this, the Parisian surgeons are however acquainted with a fact, which has, until very lately, been denied in England, viz. that every kind of ulcer is curable by common means; and M. Cullerier, the first surgeon in the Venereal Hospital at Paris, demonstrates the possibility of doing so every year to his class; but, after the ulcers have healed, he puts each patient through the usual course, to prevent secondary symptoms.

Lagneau*, the latest French author who has written on the venereal disease, although he acknowledges gonorrhœa

* Exposé des Symptomes de la Maladie Vénérienne à Paris, 1815.

may have a different origin, still endeavours to prove the indentity of gonorrhœa and chancres in the greater number of cases of syphilitic affections, from the circumstance of several females having been infected by the same man with both complaints, and the same occurring in several males, from communication with one woman, and he inculcates in consequence the propriety of treating them by mercurial preparations. In Great Britain, they are usually considered as distinct diseases; but in whatever way we are disposed to view the subject, it is hardly possible to reconcile the different facts which have been adduced, without admitting that ulcers will arise on the penis from the matter of gonorrhœa; that gonorrhœa will in turn be caused by the matter of these same ulcers, and that both occur in consequence of promiscuous or uncleanly intercourse. That many of the ulcers produced in this manner will occasionally assume every character of chancre, and cannot be distinguished from it, I am perfectly satisfied of, from repeated observation; but I am equally as certain that a gonorrhœa in men, with the worst appearances and symptoms, can, and often does, arise from irritating causes common to parts free from any specific disease or poison, is not distinguishable from one that has arisen from promiscuous intercourse, and that both complaints are curable in the same way and without mercury. It may be asked, can gonorrhœa, or the ulcers resulting from the matter of gonorrhœa, produce symptoms of constitutional derangement? The evidence of authors is at variance on this subject, and I am inclined to believe, as a general rule, that they do not; although I am by no means disposed to affirm, that they cannot, under particular circumstances of constitution, produce such symptoms; but in allowing that they do sometimes follow, I am much inclined to believe, that they become serious only in consequence of the improper exhibition of mercury; and it is from the cases that have made me allow, that secondary symptoms, such as inflammation and ulceration of the tonsils, and eruptions on the skin, do follow gonorrhœa, that I have also drawn this inference.

In the British empire, surgeons, so far from confounding these complaints, have on the contrary been long accustomed

to make distinctions between the different kinds of ulcers, to which the sexual organs are liable. Mr. Hunter endeavoured to point out the ulcer which he supposed to possess specific qualities, and to be the cause of what are called the secondary symptoms of syphilis; and since his time, the characters he has given have been supposed to be truly diagnostic, and infallible signs of a disease that required a course of mercury for its cure. His opinion has indeed so far regulated the conduct of practitioners in general, that the greater or lesser resemblance of sores to the assemblage of appearances he has described, has decided whether a course of mercury was or was not necessary: it having been taken for granted that mercury was the only cure, that a sore possessing these characters must contaminate the habit generally unless it were prevented by the exhibition of the appropriate remedy, and that it could not be cured without it. That this was, indeed I believe, is, the opinion of the most eminent surgeons in the empire, will not be denied, and I am acquainted with *none* of whose opinions records are preserved, either in their writings or by their pupils, with the exception of Mr. Abernethy, who do not allow, even if they do not support this doctrine.

Although it was supposed that a chancre was the true syphilitic ulcer, when possessing the properties I have mentioned, it was allowed, and the oldest records of surgery confirm the fact, that many other kinds of ulcers arise from promiscuous intercourse, which do not require mercury for their cure. Attempts were then made to distinguish these also; and Mr. Abernethy, Mr. Carmichael, and others, have been very successful in their endeavours. But the matter did not rest here; reference was not made alone to primary ulcers; the different secondary symptoms were also considered, and attempts again made to distinguish those resulting from each particular kind of ulcer. In this manner several diseases, supposed to depend upon different poisons, have been described; which, if they did actually exist, would be much more dangerous than syphilis itself, which all agree in thinking curable by mercury properly exhibited; whilst the other complaints are, at the same stage of the disorder, aggravated by its exhibition.

In consequence of these opinions, it became desirable to ascertain, at an early period, whether an ulcer was a chancre or not; and many surgeons prided themselves on their peculiar talent in distinguishing those ulcers, which absolutely required the use of mercury for their cure, from those that did not; but the value of this prescience will be more duly estimated now that it has been ascertained that every sore, of whatever description it may be, will heal without its use, provided sufficient time be granted, the constitution be good, the patient regular in his mode of living, and that attention be paid to cleanliness and simple dressing, and to keep the patient in a state of quietude.

During the last eighteen months, in the York Hospital, Chelsea, Mr. Dease, Dr. Arthur, Dr. Gordon and myself have been in the habit of treating all cases of ulcers on the penis, whatever form or appearance they might have, by simple mild means; that is, by dry lint, or ointments or lotions for the most part not containing mercury, in order to obviate the objection that might be made to the application of it in any form; and of near one hundred cases which have been treated in this manner, all the ulcers healed without the use of mercury; and among them there were of course many of every description, from the common ulcer, without excavation or induration, to the solitary ulcer possessing the true characteristics of chancre. Since Mr. Rose of the Guards began to treat his people without mercury, and the practice was adopted at the York Hospital, it has been followed at several of the hospital stations, at Dover, Chatham, and Edinburgh, and in different regiments at home and abroad, especially the 57th and the staff corps of cavalry in France. From these hospitals I have seen the reports of near 400 cases more, which have been treated with the same result as far as regards the cure of primary ulcers; each ulcer appears to have run a certain course, which, as to extent, was much the same as in one of the same appearance where mercury was supposed to be necessary; and at an indefinite period of time, to have taken on a healing action, and in the greater number of instances, skinned over rapidly, leaving a mark or depression shewing a loss of substance. With us, where the ulcer had the characteristic ap-

pearances of a chancre, dry lint alone was generally applied to it; where these signs were less prominent, a variety of applications were used, but there were a great number of sores both raised and excavated, on which no application made the least favourable impression for many weeks. They did, however, yield at last to simple means, after remaining for a considerable time nearly in the same state, several of them having become sores of a large size previous to, or in the first days after their admission. If they were ulcers without any very marked appearance, and did not amend in the first fortnight or three weeks, they generally remained for five or seven weeks longer; and the only difference, in this respect, between them and the raised ulcer of the prepuce was, that this often remained for a longer period, and that ulcers, possessing the true characters of chancre, required in general a still longer period for their cure; that is, from six, eight, to ten, twenty, and even in one case twenty six weeks, healing up and ulcerating again on a hardened base. Those that required the greatest length of time, had nothing particular in their appearance that could lead us to distinguish them from others of the same kind that were healed in a shorter period; neither were any of these ulcers followed by a greater number of buboes, nor did they suppurate more frequently than in the same number of cases treated by mercury; on the contrary, the ulcers were not so frequently, on the average, followed by them, neither did they so often suppurate; but this may also be attributed to the antiphlogistic means employed both generally and locally for their relief.

The fact then of the possibility of curing every kind of ulcer on the genitals without mercury seems to be fully established; but the question of time is very important, for I have every reason to be certain from former experience, that almost all these protracted cases would have been cured in one half or even one third of the time, if a moderate course of mercury had been resorted to after common applications had been found to fail; and I have reason to think, from the treatment of other cases, that the duration of many of them might also have been shortened by the regular exhibition of cathartic medicines combined with sudorifics.

The great question however is, were these people, whose ulcers were healed under this treatment, more liable to secondary symptoms than if they had been treated by mercury? According to the opinions commonly entertained, there ought not to be a doubt on the subject; but these opinions have been formed rather on what it was supposed must follow, than on what has been actually observed to follow. From the nature of the service, it has not been possible for us to trace with sufficient exactness the whole of the persons that have been treated in the York Hospital, although many remained for several months under observation; but of the whole treated, only six cases have been noticed in which symptoms strongly resembling those of syphilis made their appearance, although it is possible slighter ones not requiring medical assistance may have occurred. Of these six cases, two had ulcerated throats, combined with eruptions. In one, the papular eruption appeared before two ulcers, one a raised ulcer of the prepuce, the other a chancre on the corona glandis, had healed; one had a syphilitic leprous eruption, and being a private patient, was cured by mercury and the decoct. sarsaparillæ. Another of the same description was cured without either of these remedies.---Five of the six, then, were cured by simple means, such as cathartics, antimonials, sarsaparilla, and the warm bath, and one by the assistance of mercury.

In none of these cases were the bones affected; it is but proper, however, to remark, that several cases were admitted during this period, in which a few mercurial pills had been taken, and the mouth not been affected, and in which the primary symptoms were followed by eruptions both papular and scaly, by ulcers in the throat, by nodes, and in one case by inflammation of the periosteum covering the bones of the nose, and ulceration of the septum nasi, although mercury was resorted to for its cure; but these cases as well as many others of disease in which mercury had been frequently or irregularly used, although cured, are not included in these observations, as the exhibition of even a small quantity of it in the first instance, might be supposed by some to have a certain degree of influence on the symptoms which subsequently appeared. Mr. M'Leod, in the Hospital at Dover, out of fifty

cases treated without mercury, which he has been able to keep in view, has not had a larger proportion of secondary cases than I have. Staff-surgeon Murray, and Mr. Evans of the 57th regiment, and Mr. Brown of the staff corps of cavalry, have been equally successful in France. In the course of twelve months they treated 134 cases, and the proportion of secondary symptoms to the whole number has been under a tenth, and of the same description as my own. In Edinburgh, the result has, in 200 cases, been the same; indeed it has been so generally uniform, as far as I am acquainted, that we cannot doubt that the proportion of secondary cases of syphilis is infinitely less than is commonly supposed; but it is in all probability something greater than the preceding remarks would point out, from the cause I have assigned. It appears singular, that in the secondary cases, the symptoms should all have been of a mild nature, in two instances only affecting the bones. Some of my friends, of great talents and experience, have been induced from this to suppose, that the greater severity of symptoms, which are frequently met with, has been caused by the exhibition of mercury in the first instance, which aggravated the constitutional disease. I am rather disposed to attribute their mildness to the antiphlogistic means resorted to, on the secondary symptoms first showing themselves; because the situations I have filled have afforded me many opportunities of seeing persons suffering from the primary and secondary symptoms at the same time, where they had not taken any medicine to mitigate or impede the progress of the disease; and these were apparently running through their several stages until checked by mercury.

If we refer to Dr. Fergusson's Paper on the Venereal Disease, in the Fourth Volume of the Transactions of the Society, we shall find it stated, that in many cases in which the Portuguese certainly gave no mercury for the cure of primary symptoms, the secondary ones run their usual course even to the loss of the bones of the nose; and I am very willing to confirm a remark he once made to me, that there were more people to be met with in the town of Lisbon, who had suffered that mutilation, then in any other he has seen of the same size; we cannot then doubt that secondary symptoms of the

most serious nature will occasionally follow in particular constitutions.

The Portuguese, in treating all cases of ulcers without mercury, did only what we have been lately doing, and with nearly the same success; suffering considerable delay in the cure of the primary ulcers, and meeting with few cases of secondary symptoms in proportion to the total number treated. It ought, however, to be remarked, that ulcers of a mild character are much more prevalent in Lisbon than in London, because the lower classes of people, and especially females, have an abhorrence of cold water applied to the sexual organs. But independently of this, Dr. Fergusson supposes they did not suffer from secondary symptoms, as they ought to have done according to the ideas we then entertained, because the venereal disease was mitigated by reason of a general and inadequately resisted diffusion of it among them, in consequence of their simple mode of treating it. There is not, however, any more solid foundation for this opinion, which has, I am aware, made a strong impression on many persons in England, than there is for that which is commonly entertained that the disease is more virulent in Portugal than in Great Britain. On this point I can also only agree with Dr. Fergusson to a certain extent. I do not think the disease, which the troops contracted in Portugal, was in the slightest degree more violent than the same kind of complaint at home, neither do I place the least reliance on what has been said by others about a distemper called the Black Lion of Portugal, which I do not believe exists; but I perfectly coincide with him in opinion, that the change from the climate of Great Britain to that of Portugal in the summer, with the different mode of life, does act most powerfully on our northern constitutions, and disposes strongly to inflammatory affections. It is this that rendered the same kind of wounds more dangerous to the British soldiers than to the natives, and it was to this disposition, increased by the greatest irregularity of conduct, and often by intemperance, a vice the natives are not addicted to, that we were indebted for the mutilations which ensued from the venereal disease. If the persons affected had been managed in the manner he has informed us he treated his friend after the

battle of Vimiera, the result would have been, and was in many instances very different; but in many cases of this kind, from the strong tendency to gangrene, which is great in constitutions of this description in warm climates, mercury, bark and wine were unfortunately resorted to, instead of bleeding, and the most vigorous antiphlogistic treatment, and the part affected was destroyed; but I have seen the same thing happen in several hospitals both civil and military even in England. I write on this subject with confidence, because I was with the army during the whole war in the Peninsula, and had for several months together the superintendance of the hospitals in Lisbon, at a later period than Dr. Fergusson alludes to, and when this disease was one object of my particular attention.

I do not then think Mr. Carmichael's opinion, as to the secondary symptoms peculiar to the phagedænic and sloughing ulcer, receives any support from what occurred to the troops in Portugal; because it did not appear that either of them, following sexual intercourse, were dependent on the cause which produced the ulcer. Where many men have had intercourse with the same woman, they have not all had the same complaint, although one of the ulcers so originating has become phagedænic or sloughed; neither has the same woman herself suffered from this distemper. Indeed the nature of an ulcer of either kind must, after a short time, effectually prevent any intercourse; and we often find, that their peculiar characters only appeared after the ulcer has existed for several days. I firmly believe also, that in the greater number of cases of sloughing ulcer, where mercury is not given, no secondary symptoms would appear: and in those cases in which they did appear, I apprehend they would be equally dependent on the state of the constitution, both as to the mode of cure and their destructive characters. In other words, my observations lead me to conclude, that these ulcers do not depend upon a specific poison, but on the state of the constitution under particular excitement; and that when secondary symptoms do occur, they are not dependent on the state of the ulcer; although I am ready to admit, that in a constitution where an ulcer will rapidly become phagedænic, the second-

ary symptoms, when they do occur, may be different to a certain extent from those that follow more simple ulcers, in a healthier habit of body.

An officer, in the years 1800 and 1801, suffered severely from almost all the secondary symptoms of syphilis, such as sore throat, eruptions, and nodes, and recovered under the care of the late Mr. Rush. In the year 1808, he landed with his regiment in Portugal. In April 1809, he contracted, for a third time, an ulcer on the penis, but being obliged to move with his corps he found, at the end of the fourth day's march, that it was inflamed, red, painful, and swelled to thrice its natural size, altogether threatening the loss of the part: but from which misfortune he was saved by perfect quietude, and the most vigorous antiphlogistic measures. Whilst the army were in the lines in front of Lisbon, he again contracted an ulcer, which he concealed, until the increase of the disease obliged him to apply for advice, when he was ordered down to Lisbon. The penis was now in the same state as in April 1809, but instead of resorting to the same means of cure, he rode forty miles into Lisbon without any delay; the consequence was, the penis sloughed. He underwent several courses of mercury, but the ulceration of the penis could not be induced to heal, his health decayed, his throat became sore, and a swelling took place on the back of the hand and on the foot. He was sent to England, but in vain. He lost by ulceration the remainder of the penis, with part of the hand and foot, and at last died from ulceration of the throat, worn out by disease. His military friends said that he died of the Black Lion of Portugal; but it was clearly from the improper treatment in the first instance of a simple disease, that these dreadful consequences were induced; and I have no doubt they would have been brought on at the former period, if he had been treated in the same manner.

Having made these remarks upon the cure of diseases contracted by promiscuous intercourse, without mercury, and shown that they depend upon the state of the constitution for any peculiar malignancy, I may be permitted to revert to a former period when the same class of people were cured of the same diseases by mercury: that is, between the years 1801 and

1809, when surgeon of the 29th regiment; and I rely much on the knowledge acquired in this period, because the regimental surgeon possesses advantages as to ascertaining facts, which are not to be met with in any other walk in life. The persons affected are completely under his control, he can do what he pleases with them without restraint, and he has them under his observation for a number of years, certain that they cannot have a change of opinion, and act contrary to his wishes. In the period to which I have alluded, a great number of persons afflicted with this disease came under my care, they nearly all underwent a moderate course of mercury, provided the ulcers did not assume a healing appesrance at the end of a fortnight or three weeks, and I very seldom had a case of secondary syphilis, not even in those who were occasionally from under my inspection. I am not aware of having ever discharged or lost a man in consequence of it; and the worst case I recollect is now a coal-heaver in London, having been discharged the service, on account of a fracture of the arm, and a wound of the scrotum at the battle of Roliça. Later observation has proved, that a great number of these patients would not have had secondary symptoms, if mercury had not been exhibited; it cannot then be supposed that the mercury prevented their occurrence; but if it be not satisfactory on this point, it goes a great way to prove what is almost as interesting, viz. that mercury properly exhibited is not the cause of all the evil which in many cases is attributed to it; for if it were, these people must have given proofs of it, as every case which did not yield to simple means in the course of a fortnight or three weeks, was put upon the use of mercury. If indeed a moderate course of mercury, nay a few pills, could produce all the symptoms which are frequently supposed to arise from it, there are few of us who have lived in warm climates, where nearly all have taken it irregularly for one disease or other incidental to them, that would not be living proofs of it, while the contrary is well known to be the fact. Whether an insufficient course of mercury is more productive of secondary symptoms than no mercury at all I cannot say; but it appears to me, that it is only where mercury is persisted in after it has evidently ceased to do good, when it disagrees with

the constitution, or when it is exhibited at an improper period, or very irregularly, the patient being exposed to wet and cold, that it produces those symptoms usually supposed to depend upon it. The fact I have stated as to the non-occurrence of secondary symptoms in regimental hospitals, where all doubtful cases were treated by mercury, is so positive, that I am certain no regimental surgeon of ability will be found to contradict it; that they did sometimes occur is true, but it was only when the troops were moving and under irregular management that they were numerous, and then only in the general hospitals, where all the stragglers and all the bad or protracted cases were collected. In the half-year ending the 24th of June, near 1400 cases of primary symptoms were treated in the army in France by mercury, and in this period only 14 cases of secondary symptoms occurred. It may be said, many cases had not time to shew themselves; but then it must be recollected that all those of the preceding half-year, which required the same length of time, are included in this number, and render the calculation as correct as any computation of the kind can be. In six regiments in one district in England, 521 cases were treated in 15 months by mercury, and ten cases of secondary symptoms appeared, so that the true average proportion will be between the two, or 1 in 75. I by no means, however, wish to be understood as supposing mercury to have a good effect on all primary sores, possessing or not possessing specific characters. I am perfectly aware of its inutility in many cases, and the two following, which have lately occurred to me, would attest the fact, if the records of surgery were not already sufficient.

A gentleman perceived, after a suspicious connexion, a sore at the orifice of the urethra and two others near the frænum. For these he rubbed in mercurial ointment for five weeks, when his health deteriorating, he applied to me. I told him the two sores near the frænum would heal in a few days; but that the sore at the orifice of the urethra being dependent on situation would not heal for four or five weeks more. As his mouth was affected, I advised him to omit the mercury, and await the result. He followed my advice, and under mild applications the sores healed.

A soldier contracted an ulcer on the prepuce, and came under my care in October, 1816. At the end of five weeks, the ulcer continuing, I put him, at his earnest entreaty, on the use of mercury; his mouth soon became sore, but no amendment took place for several days in the ulcer. Three weeks after the mercury was left off, other sores of the same nature appeared; but all soon healed, with the exception of one which remained stationary. The man was now satisfied mercury would not cure him, and he waited patiently the event of his treatment, until the 18th of January, when he was discharged perfectly well.

In regard to secondary symptoms, it appears that they occur after primary ulcers which have not been cured by mercury; and that they do also occur after a well regulated course of mercury, there is no man of experience will, I believe, deny. Indeed Mr. Hunter, whose accuracy in matters of fact will not be disputed, has left us through his commentator, Dr. Adams, a most interesting case of this nature, where the disease not only affected the first, but also the second order of parts, although mercury was each time properly exhibited for its cure. This case is given as explanatory of Mr. Hunter's doctrine, that if the disposition for the disease be formed, mercury cannot cure it, until it come into action; which, in plain language, means nothing more, than that the disease cannot be prevented in certain constitutions from running its own course, when it may at last be cured.

Now this part of the doctrine, that the disposition cannot be cured until formed, and in action, is denied by many of Mr. Hunter's most enthusiastic admirers; but there is this difficulty on the subject; that we are perfectly uncertain, when a disposition has been formed, if such a thing do ever occur; and it is not a fair inference to conclude we have destroyed it, because it never shows itself in action; it is on the contrary more rational to suppose, that there never was a disposition formed at all. This conjecture may also be more reasonably entertained, now that we are aware, the greater part of these primary ulcers, which were usually supposed to be the original cause of this disposition and action, are not followed by any such effects, except under particular circumstances, with which

we are as totally unacquainted, as we are with the reasons, why one man on pricking his finger in dissecting shall have a chain of abcesses to the axilla, indicating every appearance of being infected by morbific matter, whilst half a dozen others shall in no way suffer, although exposed to the same injury, precisely at the same time, and in the same way; or, why a person, although continually exposed, and under circumstances where such an effect might be reasonably expected, should, perhaps, suffer but once or twice in his life; or why, at another time, he shall suffer in the same manner from the prick of a clean needle through a leather glove, which cannot be supposed to convey any infectious matter.

If we refer to the works of Mr. Hunter, Mr. Abernethy, Mr. Carmichael, and to the recorded opinions of Mr. Pearson and others, we shall find instances of diseases arising from accidental and constitutional causes, but without any kind of primary ulcers; indeed, instances are not wanting, of their having arisen without any sexual intercourse having ever taken place; yet these diseases so much resembled the secondary symptoms of syphilis, that it was only by the history they were supposed not to be syphilitic.

If we inquire how opinions are now formed concerning the secondary symptoms which resemble syphilis; whether from appearances or history, and what is the object of the inquiry; I believe we shall not find a surgeon in London, who values his own opinion, will venture to give one on appearances alone: all refer to history, and the object of that, to both patient and surgeon, is generally to ascertain whether the use of mercury is to be recommended or not. If the history be truly syphilitic according to our notions of cause and effect, mercury is ordered on the supposition of its being the only specific, although it has too frequently disappointed us. If we find, with the very same appearances or nearly so, that the patient has gone through several severe courses of mercury, it is very properly supposed not to be equal to the cure, or that the disease is mercurial, and sarsaparilla, warm bathing, &c. are substituted. If it arise from constitutional causes, and no syphilitic taint can be traced, even at the distance of half a dozen years, a mixed kind of course is generally resorted to.

It may be said that Mr. Hunter's supposed unerring sign of the disease in all its symptoms being progressive, never retrogressive, except mercury be used, is diagnostic of syphilis; but I do not believe this to be a fact; indeed I have already said, all these symptoms are curable without mercury; Dr. Fergusson has given proofs of it in a country where none is used, and where venereal diseases are supposed to be most prevalent; and as the same thing has been recently done in Great Britain and France, I may be excused arguing the point, because it is simply a question of fact. I deny it; but I by no means deny, on the contrary, I affirm, that in all obstinate cases of this description, after the disease is fully formed, that is, after the inflammatory stage has passed by, mercury, so long as it produces a beneficial effect, is the remedy I would use in preference to all others; but the moment this good effect was no longer progressive, that instant I would abandon it until the remedy ceased to exert an improper influence on the constitution; when I might, under certain circumstances and when a change was necessary, resume it.

If we refer to cases of the secondary symptoms of syphilis, pseudo-syphilis, or the cachexia syphiloidea, we find, that in all, a degree of constitutional derangement exists, producing symptoms so much resembling each other, as to render the adoption of a mode of cure dependent on the history of what has been already done; may we not then be induced to suppose, that neither of these states of disease depend on a specific poison circulating in the blood, and exciting particular parts into diseased action, but on a particular irritation dependent more on the state of the constitution than on the nature of the offending cause? It is allowed by all, that the secondary ulcers of syphilis, as they are called, cannot produce primary ones; a proof that the nature of the disease is changed, and that the primary and secondary disease are two distinct things.

As the irritation of a prick in the finger, only produces abscesses and general derangement when the state of the constitution is not good; as derangement of the digestive organs alone may produce in particular cases constitutional symptoms of disease resembling syphilis; as the irritation of a transplanted tooth may do so in the same manner, so am I disposed to believe, that an ulcer or syphilitic chancre produces secondary

symptoms only in particular states of the constitution: but what that state may be, or in what it may differ from a state of health, would be as difficult to describe, as in any of the other instances to which I have alluded.

Mr. Hunter has said; "the venereal matter, when taken into the constitution, produces an irritation which is capable of being continued independent of a continuance of absorption, and the constitution has no power of relief, therefore a lues venerea continues to increase." In adopting then the opinion of secondary symptoms occurring from a peculiar irritation in the constitution, I am not entertaining a new theory, I am only objecting to that part of the old one, which supposes the constitution is unable to recover itself under any circumstances without the aid of mercury; and I do so because I have had proof of it in many instances. But I would by no means imply that it either can or does in every instance: on the contrary, I object only to the opinion of a virus, absolutely requiring a specific medicine, and not to the remedy itself. Let us look upon it in the venereal disease as we regard it in other diseases, to be used only in certain cases, when it agrees with the constitution, and then with moderation and prudence, and we shall hear but seldom of the bad effects which are now so common after its exhibition.

That I may not be misunderstood in the object of this paper, I shall take the liberty of recapitulating those points on which I wish the attention to rest, as the present result of the experiment.

1. Every kind of ulcer of the genitals, of whatever form or appearance, is curable without mercury. This I consider to be established as a fact, from the observation of more than 500 cases which I am acquainted with, exclusive of those treated in the different regiments of guards, and which occurred in consequence of promiscuous intercourse.

2. Secondary symptoms, (and I exclude trifling pains, eruptions, or sore throats,) that have disappeared in a few days, have seldom followed the cure of these ulcers without mercury; and they have upon the whole more frequently followed the raised ulcer of the prepuce than the true characteristic chancre of syphilis affecting the glans penis.

3. The secondary symptoms in the cases alluded to, amounting to one tenth of the whole, and which were treated on the antiphlogistic plan, have hitherto been nearly confined to the first order of parts; that is, the bones have in two cases only been attacked, and they have equally been cured without mercury.

4. As great a length of time has elapsed in many of these cases without the occurrence of secondary symptoms, as is considered satisfactory where mercury has been used, viz. from six to eighteen months.

5. The primary sores were of every description, from the superficial ulcer of the prepuce and glans to the raised ulcer of the prepuce, the excavated ulcer of the glans, and the irritable and sloughing ulcer of these parts. In the inflammatory stage, attended by itching, scabbing, and ulceration, they were treated for the most part by antiphlogistic and mild remedies; in the latter stage, when the ulcers were indolent, whether raised or excavated, by gentle stimulants.

6. The duration of these stages is very different, is often increased by caustic and irritating applications, and is much influenced by surgical discrimination in the local treatment.

7. The last, or indolent stage, often continues for a great length of time, especially in the excavated chancre and raised ulcer of the prepuce; and it appears to me, that in these particular cases a gentle course of mercury, so as slightly to affect the gums, will materially shorten the duration of it, although in others it is occasionally of no service.

8. Although the secondary symptoms do for the most part yield to simple remedies, such as venæsection, sudorifics, the warm bath, sarsaparilla, &c. without much loss of time, that is, in the course of from one to four and six months; yet, as in the primary ulcers, a gentle course of mercury will frequently expedite, and in particular persons and states of constitutions is necessary to effect a cure; and that a repetition of it will even, in some cases, be requisite to render it permanent.

Much satisfactory information is yet to be acquired, many experiments to be instituted, and much patient investigation to be gone through, in the comparative treatment of these dis-

cases with and without mercury, before we can arrive at any fair conclusion on a subject of such great importance. It appears, for the reasons I have already assigned, that it is to the surgeons of regiments we are principally to look for them; and from the attention which is bestowed by Sir James M'Grigor, the Director general of the medical department of the army, to this subject, there is every reason to think that much will be done in the course of the next few years. In the mean time, it is not my intention to recommend that the practice should be indiscriminately adopted in private life; but advantage may be taken of the facts I have stated on many occasions, to the essential benefit of the patients. In persons of a strumous habit, in those with whom mercury is known to disagree, in others who are supposed to labour under its effects, or of it and the disease combined, and who imprudently contract ulcers resembling chancres, and to whom a course of mercury might, according to received opinions, be highly detrimental, it must be very satisfactory to know that these ulcers will heal by simple means; and that if they be regular in their mode of life, secondary symptoms may not follow, or if they should, that there is still a probability of their being cured without the use of that remedy, which to these people may prove a greater scourge than the disease for which it is administered.

The facts I have adduced must necessarily lead many to pay greater attention to the nature of the ulcers they continually meet with, and may induce some few to repeat the experiments for their own satisfaction: but, before the practice can become generally useful, the minds of medical men must be better satisfied of its validity, so that a hasty change of opinion may not lead to a material change of proceeding, that the efforts of one man may not be counteracted by the mere opinions of another. But I again repeat, if any one should be disposed to try the method of cure, the effects of which I have noticed, let him constantly bear in mind, that every case so treated, requires as much attention and quietude on the part of the patient, and more attention and discrimination on the part of the surgeon, than when mercury is used for the cure. If any one should suppose, from what I have said, that there is no such a thing as a venereal disease, that the ulcers on the

penis are all common sores, requiring no more care or attention as to diet, exercise, regularity of life, cleanliness, or dressing, than an ulcer on the arm or other part of the same size, occurring from an accidental cause, he will find himself very much mistaken; and the result of his trials will be a more frequent recourse to mercury, and a longer continuance in its exhibition, than is even customary at present.

I beg it may be remarked that I have not given an opinion on, or entered at all into many important points inseparable from a due consideration of the subject; my object has only been to state a few facts, and make some observations on them. If they had been more comprehensive, I should have had to apologize for a volume instead of a paper.

SELECTED REVIEWS.

Analysis of Opium, particularly of Morphium and Meconic Acid, considered as essential parts of Opium. By M. SERTUERNER, Pharmacopolist at Eimbeck, in the Kingdom of Hanover*.

[From the London Medical Repository, for November, 1817.]

"FOURTEEN years have nearly elapsed since M. Derosne, an apothecary at Paris, examined opium, and published the results of his labours in the *Annales de Chimie*, vol. xlv. I was engaged in an analysis of the same substance much about the same time; but the results of our experiments were so different and so contradictory, that the subject has remained undetermined. Little attention has been paid to my Memoir; it was written in a hurry: the quantities were small upon which I had operated; and some persons had repeated my experiments without success. Convinced of the exactness of my labours in general, although undertaken at an inexperienced age, and desirous of removing contradictions, I engaged myself in a second analysis of this singular vegetable substance; and have had the satisfaction of confirming my former observations and making new ones. It will be seen in the sequel, that the method and the observations of M. Derosne in the analysis of opium were not exact; and that he has not discovered the part upon which the efficacy essentially depends; for he has mistaken for this substance, which I call *morphium*, its combination with the *acid of opium*. In making my observations public, I flatter myself that chemists and physicians will derive some benefit from them. They will throw a light upon the characteristics of these two bodies and the constituent parts of opium.

* Gilbert's Annalen der Physik, neue folge, vol. xxv. p. 56.; and Annales de Chimie et de Physique, t. v. p. 21.

I believe also that I have enriched chemistry with the knowledge of a new vegetable acid (the meconic acid), and with the discovery of a new alkaline base, morphium. The latter is a very singular substance, and seems to have some resemblance to ammonia, and which, some day, will serve to explain the other salifiable bases. Although my former observations upon opium and its constituent parts are confirmed, there are also some facts which I have found to be different: but I hope to be pardoned my former errors, when my youth and the small quantities operated upon are taken into consideration.

"I. *Of Morphium.*—1. Eight ounces of dried opium were digested in the heat with several portions of distilled water, until it became no longer coloured. After having evaporated the tinctures, we obtained a translucid extract, which became very turbid on diluting it with water; but with the assistance of heat, or by adding a still larger quantity of water, the transparency of the liquor was restored. The aqueous solution of the extract was saturated with an excess of ammonia, and a substance of a greyish-white colour was precipitated, which formed into translucid granular crystals. These crystals, after being washed with several portions of water, are *morphium*, the efficacious part of opium, combined with a little extract and meconic acid, as will be seen in the sequel of this Memoir.

"2. This substance, after being dried, weighed sixteen drachms. It was saturated with diluted sulphuric acid, in slight excess, precipitated afresh with ammonia, and treated several times with this alkali, in order to separate the extract. But as this could not be effected, the precipitate was reduced to an impalpable powder, and treated several times with a little alcohol, which became very much coloured. By this means I obtained nearly eight drachms of nearly colourless morphium.

"3. The small quantity of morphium which was dissolved by the alcohol, was separated by crystallization. The extracted matter which was contained along with the morphium, in the alcohol, as well as in the ammoniacal liquors, was not pure extractive, but a combination of extractive with morphium, of which the latter was the base. It is very soluble in acids and

alcohol; but sparingly so in water. It coloured the salts of iron, green, by means of its extractive, and separated from them a part of the oxide by its morphium. As pure morphium is precipitated from its solution in acids in form of a pearly powder, and since it crystallizes in parallelopipeds with oblique faces, I presumed that it is the extractive, combined with the morphium, which changed its form into an almost cubical grained crystallization. This is confirmed by treating this substance with ammonia, which dissolves a part only of the extractive, as it has something of the nature of an acid, always retaining some of the morphium, and making it difficult to separate them completely. Alcohol finished the separation, and dissolved the extractive which remain with the morphium. There is a very great difference between the extractive matter separated by ammonia and that which is separated by alcohol. The first dissolves in water with great facility, because it contains less of the morphium than the brown substance obtained by means of an alcohol: in the one, extractive predominates; and in the other, morphium. Hence we give the former, by means of an alcoholic solution of morphium, the appearances of a resinous body, in which the morphium predominates and performs the functions of a base. A concentrated aqueous extract of opium always gives these two combinations when treated with ammonia.

" 4. In order to obtain morphium in its highest state of purity, I dissolved it several times afresh in alcohol, and obtained it, by means of crystallization, entirely colourless, and in regular parallelopipeds with oblique faces. The substance which is obtained by treating opium with alcohol, according to Derosne, crystallizes in prisms, with angles from 30° to 40°, and reddens in a high degree the solutions of iron.

" 5. Pure morphium possesses the following properties: —It is colourless, soluble in small quantities in boiling water, very soluble in alcohol and in ether, and may be made much more so by the application of heat. Its solutions, from which it crystallizes in the above-mentioned forms, are very bitter. The aqueous and alcoholic solutions tinge rhubarb paper brown more strongly than turmeric paper, and restore the blue colour of litmus paper after it has been reddened by acids. It

is not ammonia which produces this effect, because pure morphium does not contain any of it, as will be seen in the sequel by treating this substance with caustic potash. It dissolves with facility in the acids, and forms very remarkable neutral salts.

" The *sub-carbanate of morphium* is formed either by exposing morphium to the action of carbonic acid, or by decomposing the solution by sub-carbonate of potash.

" The *carbonate of morphium* crystallizes in short prisms.

" The *acetate of morphium* crystallizes in small rays, and is very soluble.

" The *sulphate of morphium* is very soluble, and crystallizes in ramifications.

" The *muriate of morphium* forms plumose or radiated crystals, and is more sparingly soluble than any of the other salts with morphium; and, if the evaporation be pushed too far, cools into a brilliant silver-white mass.

" The *nitrate of morphium* crystallizes in rays, proceeding from a common centre.

" I have not formed the *meconate of morphium*; but the *sub-meconate* crystallizes in prisms; and I obtained it by treating the aqueous solution of opium with alcohol. A large portion of water is necessary to dissolve it and separate it entirely from opium.

" The *tartrate of morphium* crystallizes in prisms, very similar to those of the preceding salt.

" These different salts formed with morphium appeared to me to be very hurtful to the animal constitution; for, after tasting them, I was always troubled with a head-ache. They are very soluble in water; almost all have a micaceous brilliancy, and very soon effloresce when exposed to the air.

" In the order of salifiable bases, morphium will stand after ammonia, because it is disengaged from all its combinations by this alkali. It will occupy the last place among the alkalies, from which it is distinguished in having less specific gravity, and in being capable of saponifying oxydized oils. It has less affinity for acids than ammonia, and even than magnesia; but it decomposes most of the metallic salts, as the sulphate, the muriate, and the acetate of iron, several of the salts of mercury, lead, and copper.

"The acetate of morphium loses its green colour by means of morphium, and, no doubt, forms with it a triple combination, as with ammonia. It combines with the carbonic acid of the atmosphere and with extractive, as the other salifiable bases.

"Morphium easily melts by the application of heat, and then has some resemblance to melted sulphur: it crystallizes upon cooling. It burns very quickly; and, by heating it in a close apparatus, a blackish resinous substance may be obtained, of a peculiar odour. It combines with sulphur when heated; but is at the same time destroyed, and forms hydro-sulphuric acid. I have not had time to determine exactly the elements of morphium: but there is no doubt that they are oxygen, carbon, and hydrogen; perhaps also azote. The galvanic pile produces little effect upon morphium, even in making use of a globule of mercury. The globule, nevertheless, seemed to enlarge and alter in consistence.

"II. *Of the Effects of Morphium on the Human Body.*—6. The most remarkable property of morphium is the effect which it produces upon the animal œconomy. That I might ascertain this with exactness, I tried experiments upon myself, as well as upon some other persons, being convinced that experiments made upon the lower animals do not afford any certain results.

"It is necessary to notice in a particular manner the terrible effects of this new body, in order to prevent accidents; for it has been publicly declared that this substance has been given in considerable quantities to several persons without producing any remarkable effect. If it were really morphium that was given in these cases, it would seem that it could not have been dissolved by the gastric juices. My former experiments induce me to mention expressly that this substance should only be given when dissolved in alcohol, or in a little acid; because it is very sparingly soluble in water; and that consequently it cannot be readily acted upon in the stomach without the medium of these liquids. That I might ascertain with exactness my own sensations, I engaged three persons, none of whom were more than seventeen years of age, to take the morphium along with me. But, warned by the effects which I had for-

merly experienced, I gave to each only half a grain, dissolved in half a drachm of alcohol, diluted with some ounces of distilled water. A general redness, which might even be perceived in the eyes, covered the whole body, but particularly the cheeks; and the animal powers seemed to be raised. We then took, half an hour afterwards, another half grain of morphium. The effects already mentioned increased considerably, and we experienced a transitory inclination to vomit, with a sense of stunning in the head. Without waiting the issue of this, we swallowed, a quarter of an hour afterwards, another half a grain of morphium, in coarse powder, with some drops of alcohol, and half an ounce of water. The effect of this was instantaneous upon the three young men; they experienced an acute pain in the stomach, a sense of weakness, with general stiffness, and faintness. I myself experienced similar effects; and, upon lying down, fell into a kind of *dozing reverie*, attended with a sensation of throbbing in the extremities, but chiefly in the arms.

"These evident symptoms of poisoning, and especially the faint state of the young men, occasioned me so much uneasiness, that, without any consideration, I swallowed about six or eight ounces of very strong vinegar, and gave the same quantity to the others. So violent a vomiting succeeded, that one of the young men, of a delicate constitution, and whose stomach was entirely empty, found himself in a very miserable condition. It appeared to me that the vinegar communicated to the morphium this violent emetic property.

"I then gave this young man some carbonate of magnesia, which soon checked the vomiting; and he passed the night in profound sleep. Next day the vomiting returned; but soon ceased upon taking a strong dose of the carbonate of magnesia: but the want of appetite, the constipation, stiffness, and pain in the head and stomach, did not cease for some days.

"So, if we may judge from this rather disagreeable experiment, morphium, even in small doses, is a violent poison. Its combinations with the acids may, perhaps, possess still more powerful effects. I am of opinion that the last half grain had a more energetic action, from arriving in the stomach in a concentrated state, and being there dissolved.

"The other constituent parts of opium do not possess any of the above mentioned properties; and it appears to me that the principal effects of opium depend upon the pure morphium. It belongs to medical men to investigate this subject. Hitherto the combinations of morphium with meconic acid only have been employed. We may also expect efficacious effects in several diseases from the different salts of morphium. From my own experience, I am able to say, that a very violent toothache, which would not yield to opium, gave way upon the application of a weak alcohol solution of morphium.

"The meconate of morphium, which constitutes the efficacious part of opium, being rather insoluble in water, pure alcohol only ought to be employed for making the tinctures of opium. These tinctures ought also never to be cooled too much; because, when this is the case, morphium, combined with some of the resinous parts, is precipitated from the extractive and the meconic acid; and hence such remedies are less efficacious at a very low, than at a moderate temperature. It would be desirable that medical men, competent for the task, would soon engage themselves upon this subject; because opium is one of our most powerful remedies.

"III. *Of the Acid of Opium or Meconic Acid.*—7. Having shown the properties of morphium, we will now examine the liquid from which it was precipitated by the ammonia. After having evaporated it to the consistence of a syrup, a little morphium in irregular crystals was deposited. Ammonia caused a precipitate, which was almost entirely morphium, but which is dissolved in the extractive if the ammonia be disengaged by heat. This extractive has the characters of acidity; but it cannot retain ammonia at an elevated temperature, and combines with morphium; which combination we shall shortly notice. After having evaporated a little morphium by an excess of ammonia, the extract of opium was filtered, dissolved in distilled water, the ammonia disengaged by heat, and a solution of muriate of barytes added until there ceased to be any precipitate. The precipitate, washed and dried, weighed six drachms. It is a quadruple combination of barytes, morphium, meconic acid, and extractive, sparingly soluble in water.

"8. I endeavoured to separate the morphium from the

extractive by means of alcohol, and to leave the meconic acid pure in the liquid, by saturating the barytes with a sufficient quantity of sulphuric acid. I succeeded in obtaining the meconic acid by evaporation; but, as it was coloured, I was obliged to subject it to sublimation. It melted first in its water of crystallization, and then sublimed in fine long needles. It was colourless, of an acid taste, had all the properties of the strong acids, and distinguished itself by its strong affinity for the oxide of iron, which it precipitated from its muriatic solution, of a cherry-red colour, even when there was a slight excess of acid. But it did not indicate the presence of iron in the ferrugineous prussiate of potass, as I had at first thought, from being deceived by the high colour of the acid which I had employed. The apparatus unfortunately burst during the sublimation, and the small quantity of acid which I had obtained was by this means still more diminished, so that I was prevented from examining the salts which it formed with the different bases. I combined it only with lime, forming the meconate of lime, which is an acid salt, crystallizes in prisms, is rather insoluble, and is not decomposed by sulphuric acid.

" The meconic acid does not produce any sensible effect upon the human body. I have taken five grains of it without experiencing any inconvenience. It appears to me, that it weakens the effects of opium, and renders it more soluble in water, as indeed do all the acids; and I am of opinion, that this property of the acids is owing to their forming salts with morphium with an excess of acid; but we nevertheless find, that the other salifiable bases are often more hurtful to life in combination with the acids, than when taken in their simple state; and perhaps this may be the case with the salts of morphium.

" 9. The liquid from which I had separated the morphium and the meconic acid was tinged of a red colour by the muriate of iron and by sulphuric acid. By evaporating it to the consistence of syrup, I obtained forty grains of a rather insoluble salt, from which I separated a little morphium by means of alcohol, which had been dissolved by the water and was precipitated during the evaporation. By treating this salt with sulphuric acid, a sulphate of barytes and meconic acid were

formed; this salt, therefore, must have been meconate of barytes.

"IV. *Of the other parts soluble in water.*—10. Alcohol having dissolved so little of the morphium from the quadruple combination, I thought it must have been retained by the extractive; and indeed the extract from which I had separated the meconate of barytes, after being diluted with water, and evaporated to the consistence of syrup, precipitated about thirty grains of a mass, which I recognised for a combination of extractive with morphium, and which was dissolved by the alcohol, with the exception of a little meconate of barytes which had combined with it. Thinking I had obtained some extractive, I gave ten grains of it, in several doses, to one of my pupils, who soon brought it up by vomiting. A little ammonia formed a slight precipitate in it, which disappeared upon expelling the ammonia by heat. I repeated this experiment several times. The precipitate which was formed in it when cold, by the ammonia, had the properties of morphium, and combined with the extractive when the ammonia was expelled by heat.

"These results determined me to re-dissolve the extract in water. When this was thickened as usual, I gathered a little of the precipitate by filtration; it had the appearance of morphium with much extractive, it was dissolved by alcohol, and there were some crystalline traces of morphium. When I added the ammonia in excess, the precipitate was very copious, and almost the whole mass became ductile and resinous; which, in a dose of five grains, produced the same effects as the extract of opium, only in a smaller degree. This substance was very little soluble in cold water; it decomposed the metallic salts like morphium; it was soluble in the acids by saturating them, and ammonia, which dissolved a good deal of it, deposited a greyish mass, which was morphium combined with a little extractive. I endeavoured to precipitate the extractive with the morphium by means of sub-acetate of lead, and to separate the morphium from it by means of alcohol; but I obtained only a little morphium, coloured by the extractive; the remainder seemed to me to have formed a triple combination. When I had decomposed the precipitate with sulphuric acid, the extract

still possessed its hurtful influence, and, by the addition of ammonia, showed the presence of a resinous substance. It seems to me, that morphium has a great affinity for extractive which is perhaps highly oxidized, and forms different combinations with it; one containing a great deal of morphium, of which the crystals mentioned above are formed; the other containing a good deal of extractive, which is precipitated by the ammonia like a resinous substance, from the extract of opium separated from its acid and from the morphium which was dissolved in it. Although the oxidized extractive was combined in excess with the morphium, the combination, nevertheless, possessed its principal properties; it was little soluble in water, very soluble in alcohol and the acids; it saturated the latter, and was precipitated by ammonia, which, combining with the excess of extractive, formed a substance soluble in water. The extractive, however, which combines with the ammonia, still contains morphium. I confess that I ought to have attempted still more to have separated the morphium completely from it by means of æther, the oil of turpentine, or *concentrated* alcohol.

" A combination of extractive with morphium may be formed artificially, by dissolving the morphium with alcohol and treating it with the extractive of some other substance. It belongs to the properties of morphium and of extractive of combining together, because one possesses the character of a base, and the other of an acid.

" V. *Of the parts which are not soluble in water.*—11. Not having digested sufficiently the opium in water, I presumed that the remainder of the opium still contained morphium and meconic acid. I treated it with half an ounce of weak muriatic acid and a sufficient quantity of water. After having filtered the liquor, I added ammonia to it. I obtained, without counting what remained in the liquid, about two drachms of morphium, combined with a good deal of extractive and a pulverulent substance. The ammonia which had been added in excess was disengaged by heat. The liquid, treated with a solution of muriate of barytes, gave by evaporation a small quantity of meconate of barytes.

"12. The residue from which the extractive, morphium, and the meconic acid, had been separated, by means of water and

of muriatic acid, weighed one ounce five drachms; it was almost clammy. I treated it with alcohol until it was no longer coloured. By adding water and distilling the alcohol, I obtained a brown substance, which had the appearance of a balsam, swam upon water, and was almost insoluble in alcohol. It had the odour of fish dried in smoke, and the taste of fat; it burned with a flame, which deposited a great deal of soot; and did not produce any effect upon me or other persons, even in doses of twenty grains. A small dog swallowed some drachms of it without experiencing any pernicious effects. I digested one part of this residue with oil of turpentine, and another with sulphuric æther; and I obtained, by the evaporation of the solvents, a kind of caoutchouc, which appeared to me still to retain a little of the brown substance, especially that which I had obtained by the essence of turpentine. By treating the residue of the opium, deprived of all the soluble parts, with diluted sulphuric acid, I changed it into a mucous substance.

" VI. *Results afforded by the treatment of opium with cold water.*—13. As heat might have had some influence upon the results of my experiments, and not having as yet explained all that Derosne says of opium, I varied them in the following manner. 1000 grains of powdered opium were triturated in a porcelain mortar with small quantities of cold water, which was added at several distinct times. After some hours the liquor was filtered, the opium strongly pressed, and this treatment was continued until the water appeared colourless. This very diluted aqueous tincture, evaporated very slowly, gave an extract different from the first, in not being at all disturbed upon the addition of water. But ammonia and the ferruginous salts equally indicating the presence of meconate of morphium, I think that it is an acid combination of these two new bodies, because the tincture of litmus was reddened by it. Alcohol does not decompose this salt; extractive is dissolved in it.

" 14. The opium extracted by cold water was boiled for a quarter of an hour in a little water. The filtered liquor became very thick upon cooling, and had the appearance of a decoction of cinchona, though not so high coloured, and acted like the sub-meconate of morphium combined with a little

extractive. It was deposited upon the sides of the vessel in the form of a brownish mass, in which, after some time, prismatic crystals of meconate of morphium were produced.

" 15. The residue was digested with alcohol at an elevated temperature. The brownish liquor filtered deposited, after having been cooled to 5° C., the salt described by Derosne. This salt, by means of its morphium, restores the colour of reddened litmus paper, and has a weak action upon the ferrugineous salts by its meconic acid. At the same time there was deposited at the bottom of the vessel a coloured substance, which, when treated with alcohol, gave a little sub-meconate of morphium. The residue, which was very soluble in alcohol but almost insoluble in water, contained the combination of morphium with the extractive and the brown substance, which has been mentioned before (12). The combination of morphium with extractive is distinguished from resins by this property; also the extractive can be separated from it only with difficulty. Ammonia dissolves still less of it, and diminishes its solubility in water. The remainder, treated once more with alcohol, gave a coloured tincture, which was much thickened by adding water, and did not regain its transparency upon the addition of acetic acid; hence it follows, that the precipitate comes from an oil. The solution contained so little of the combination mentioned above, that the taste was scarcely bitter.

" 16. It follows from these observations, that cold water dissolves the meconate of morphium with excess of acid, and almost all the extractive; and that the residue contains submeconate of morphium, which is rather insoluble in water, with a little extractive, which is easily dissolved by cold alcohol, but is almost all deposited in crystals.

" *Conclusions.*—17. Without noticing the accidental mixtures which have not been taken into account in this place, but of which I have spoken in my former researches, the *crude opium of commerce* is composed of *meconate of morphium* with little acid, which is divided by warm water into *insoluble submeconate of morphium* and into *acid meconate of morphium*, very soluble in water; supposing that there are no other vegetable acids intermixed, which may redden the litmus paper. The

extractive, like the morphium, is divided into two portions; of which, one, which is free, dissolves in cold water; the other part, undoubtedly never oxidized, remains in the residue with the sub-meconate of morphium, and forms by digestion with alcohol some sub-meconate of morphium, and a combination of morphium with extractive, almost insoluble in water, but very soluble in the acids.

"Water, when warm, dissolves more morphium than when cold; and morphium is precipitated in the cold from the liquor in combination with a little meconic acid and extractive. The resinous substance and the other constituent parts of opium, have no influence upon its medical virtues; because they are almost insoluble in water as well as in alcohol. Hence there is a great difference between the extract of opium which is prepared with warm water, and that which is prepared with cold; the latter being much more powerful than the former. The tinctures of opium ought to be prepared with pure alcohol, because by it only are the combinations mentioned above dissolved. These tinctures ought not to be kept in places where the temperance is too cold, because some of the salt of morphium will be precipitated. These inconveniences might be obviated by adding a little of the acetic acid, if we were sure that the acetate of morphium possessed the same medical properties as opium, or the meconate of morphium.

"The extract of indigenous poppy-heads prepared with distilled water, gave no signs of morphium when treated with ammonia, even when acetic acid was added. It appears, that this plant contains morphium combined with extractive. I could discover no traces of meconic acid in it. Other chemists who have examined opium, seem to have obtained different results.

"Appendix.—I had proceeded thus far in my memoir, when I had occasion to make the following observations upon the preparation of morphium and meconic acid:

"1. Take eight ounces of opium in powder, rub it up with two or three ounces of concentrated acetic acid and a little distilled water; dilute the paste with two or three pounds of cold water, and filter the liquor. This slightly coloured solution con-

tains acetate and meconate of morphium, extractive, and traces of the combination of extractive with morphium.

" 2. Precipitate the morphium with ammonia, and evaporate the liquor to a fourth part; upon its cooling, separate the morphium by filtration, and precipitate the meconate of ammonia by a sufficient quantity of acetate of barytes. Upon evaporating the liquid to dryness, there is still deposited a little meconate of barytes. The extract from which the acetates had been separated by concentrated alcohol, is almost pure extractive. I have taken it in a dose of ten grains without the least inconvenience.

" 3. The residue (1) contains the combination of extractive in excess with morphium, which is rather insoluble in water. I treated it at several times with sulphuric acid diluted with six parts of water; and I decomposed the acid solution by ammonia. This decomposition was not complete, for there always remains some morphium with an excess of extractive, of brown meconic acid, and a trace of sulphuric acid. This *acid extractive*, or *brown meconic acid*, is without effect: it is morphium which communicates to it its pernicious properties.

" *Conclusions*.—Crude opium then contains *neutral extractive* and *acid extractive*, which both exert no effect upon the animal œconomy. The latter is combined with morphium; but it combines also with the meconate of morphium, and becomes soluble in water. This combination is decomposed only in part by the water; for the residue contains traces of meconic acid, which, forming a triple combination with a large quantity of morphium and extractive, dissolves in water only by degrees. This is the reason why the extract of opium prepared with cold water, contains only a part of the meconate of morphium, and a little of the combination of morphium with the extractive. By adding acetic acid, this combination loses a part of its morphium; and the meconate of morphium separates from the brown meconate of morphium."

A Practical Inquiry into the Causes of the frequent Failure of the Operations of Depression and of the Extraction of the Cataract, as usually performed; with the Description of a Series of new and improved Operations, by the Practice of which most of these Causes of Failure may be avoided Illustrated by tables of the comparative Success of the new and old Modes of Practice. By Sir WILLIAM ADAMS, Member of the Royal College of Surgeons in London, Oculist Extraordinary to his Royal Highness the Prince Regent, &c. London, 1817.

[From the Annals of Medicine and Surgery, December, 1817.]

PERHAPS no regular oculist acquired in the same short period such high popularity as Sir William Adams, and certainly in the midst of popularity never was any one before so bitterly assailed. Without the assistance of any private interest in either the fashionable or the commercial world, to which, not to merit, too many of our profession owe their elevation, Sir William Adams has worked his way, acquiring friends by his own exertions, from insignificance to the rank of the first oculist of London. But from his very outset he has had constant warfare. First accused of ingratitude by his master, he has been ever since attacked by some one or other in pamphlets, journals, letters, and conversation; called ignorant, ungrateful, illiberal, arrogant, and ten thousand other hard names, for which the great revenue of his practice does not appear to us, with our fine feelings, any thing like a compensation.

Seeing him thus extraordinarily distinguished in two very opposite respects, we may fairly draw two general conclusions, that he must have some degree of professional merit, and that he has some faults of character; that at any rate he must be an industrious man, and that at least he perhaps is over-eager and speaks his mind too freely. We have it however now in our power to decide more minutely—to ascertain whether he has the merit of improvement and originality, and whether his faults are of a darker stain.

The work upon cataract consists of four chapters, of which we purpose to give a full analysis.

I. *History and Nature of Cataract.*—After stating the erroneous opinions of the ancients in regard to the seat of cataract, and the discovery of its real nature in the seventh century, after the celebrated astronomer Kepler had shown the transparency of the lens, the abandonment of this discovery, and the revival of the truth in the beginning of the eighth century; he proceeds to the history of the disease, and mentions an instance, under his own observation, of the complete ossification of the lens. A quotation is given from Pliny (lib. xxv. cap. 13.), which proves, that the ancients applied vegetable matters to the eyes for the purpose of dilating the pupils, previously to the operation of couching. "Pupillas dilatat, et ideo hoc inunguntur ante, quibus Paraceuthesis sit." We could almost wish this remark had come from some other person, rather than our author, who makes it, however, naturally enough, when urging the necessity of dilating the pupil to learn the existence of that species of cataract which depends on the opacity of the posterior part of the capsule of the lens,—a species too frequently mistaken for amaurosis, and entirely unnoticed by many eminent writers,—particularly mentioned in Sir W. Adams's former work, after the successful treatment of a remarkable case of that kind,—and subsequently spoken of by Mr. Travers.

The belief of Mr. Travers, that the lens is enclosed in a duplicature of the vitreous membrane, he shows to be in opposition to Richter, Janin, Scarpa, De Wenzel the younger, Ware, De Gravers, and Zinn; and contends, from facts which he himself has witnessed, that the lens has a capsule of its own.

"In operations for artificial pupil, performed on two ladies five years ago, the capsule of the crystalline in each was so hard and thick, that, although I had no difficulty in dividing the iris with the artificial pupil knife, yet it made no kind of impression on the capsule of the lens; which, with its contents, became spontaneously depressed below the margin of the pupil as soon as it was detached from the posterior part of the iris. To this time no alteration whatever has taken place ei-

ther in the size or form of these opake lenses, which certainly proves, that they must be enveloped in the capsulæ propriæ; otherwise, the vitreous tunic being disorganized in a great degree, as shown by the morbidly fluid state of the vitreous humour, admitting of the spontaneous depression of the capsule, they would have long since become dissolved and absorbed.*

"The very different manner in which the tunica propria of the lens and the tunic of the vitreous body are constituted, conclusively prove that they are totally distinct membranes; for there is no difficulty in injecting the vessels of the former, while, as far as I have been able to learn, no one has ever succeeded in injecting the vessels of the latter. Indeed, without some artificial process, such as boiling, freezing, or putting the vitreous body into acids, the vitreous capsule cannot even be distinctly perceived. Moreover, I can assert, from repeated observations, that in cases where the capsule of the lens has been most dense and thickened by disease, that of the vitreous humour has been either partially or wholly disorganized; thereby showing, that an opposite action had taken place in the two membranes at the same time, and in the same eye. Now, on the contrary, in such cases, were Mr. Travers's opinion correct, we should never find any crystalline lens within the capsule; as the disorganized state of the vitreous humour, consisting in the total dissolution of its capsule, the supposed "duplicature" must become disorganized with the other parts of that membrane, when the lens would necessarily either fall into the anterior chamber or become depressed in the vitreous humour."

We unhesitatingly agree with Sir William Adams.

Four cases of black cataract have occurred to our author; three of which had been pronounced cases of amaurosis by a late celebrated oculist, and on three of which Sir W. A. operated with success.

* "I have lately seen a very remarkable case, where the capsule, with its lens, became spontaneously separated from its natural adhesions, and has remained in a disorganized vitreous humour, depressed and undissolved, below the axis of vision, for nearly twenty years."

In opposition to Scarpa, he maintains that an experienced oculist would judge very accurately of the species of cataract by the appearance.

Arguments are very successfully adduced to prove that cataract is never to be regarded as the result of an inflammation of the lens, but rather of a want of blood—of a deficiency of vitality. But in this our author has been altogether anticipated by Delpech, who, in his great work reviewed in our fifth Number, in which the whole passage is quoted, argues this point at considerable length, and concludes with the following words: "If these considerations are corroborated at some future period by new and conclusive facts, we should be justified in thinking that the yellow colour of the crystalline is a sign of decrepitude; that the spontaneous cataract of old men is a phenomenon analogous to the falling out of the teeth in old age; and that the spontaneous cataract of young persons is the sign of a kind of anticipated old age, and of a weak constitution of the organs of vision."

After a variety of remarks upon the different kinds of cataract, the impossibility of curing the disease, unless in specific instances, by internal remedies; and the difficulty of knowing whether confirmed amaurosis be conjoined with the cataract; this excellent chapter terminates with the practical rule always observed by the writer.

"It is, and has long since been a rule with me, to advise an operation, whenever a cataract is found to exist, whether complicated with adhesions to the iris or not, if the patient is capable of perceiving light from darkness, and the motion of an opake body between his eye and the light; for, although I have sometimes been disappointed in my expectations of the result of operations when undertaken under such circumstances, yet, in by far the largest proportion of cases, the attempt has been successful in restoring vision in a greater or less degree.

II. *History and Nature of Depression.*—Depression is described by Celsus; and the native Indians perform it, although, as shown by Dr. Scott, in a different manner. As the inhabitants of India are of much greater antiquity than the Greeks or Romans were, there could be no reason for supposing they

learnt the art from either, even had they operated after the manner of Celsus. They are said to possess the works of Hippocrates, Galen, &c. We need not remark, that the Indians appear to be of the same nation as the ancient Egyptians—the first civilized people of the earth, who carried the division of labour so far, that every disease was treated by different practitioners.

Couching fell into disrepute from its frequent ill success, till Pott, Scarpa, and Hey, revived it. The former, Sir W. shows to have first stated the power of the vitreous and aqueous humours to dissolve the lens when displaced, notwithstanding Professor Scarpa would claim this merit for himself. (*Pott's Works*, vol. iii. p. 157 et seq.)

The following are our author's objections to couching:—

"In the first place, there is no kind of cataract, the nucleus of which is capable of being divided, that is not much more speedily and certainly removed by absorption, when placed in the anterior chamber, than when depressed in the vitreous chamber. 2. When the vitreous humour is in a healthy condition, every depressed cataract is liable, at any time before its dissolution, either to rise and resume its former situation, or to fall into the anterior chamber of the eye; where, from being undivided, it may occasion great pain and suffering to the patient, and even hazard the total destruction of the organ. 3. When a hard and solid cataract, whether its capsule be lacerated or not, is depressed into a disorganized vitreous humour, unless the nucleus be divided, it will remain so long undissolved, as by its weight and pressure on the retina, frequently to cause gutta serena; or by its rolling about, from the motion of the head, and the continual friction on the retina or iris, to occasion such severe pains and inflammation, as to defeat the purpose of the operation, by producing a closure of the pupil, or a suppuration and sinking of the eye."

These objections are enlarged upon with considerable ability.

III. *History and Nature of Extraction.*—This operation is usually thought modern, but Sir W. gives a quotation upon it from Pliny, and another from Continens Rhasis. Dr. He-

lenus Scott, also, informs him that the native Indians are acquainted with it.

It is objected to, 1. Because inapplicable to children or adults born blind, from their inability to retain the eye fixed; 2. Because the capsule may be opake, and to extract it, especially if morbidly adherent, is very difficult; 3. Because the cataract may be fluid, in which case, moreover, the capsule is generally opake; 4. The cornea may be so flat, and the iris so prominent, as to leave no room for the knife; 5. The eye may be too much sunk in the head for the operation; 6. Extraction often causes such violent spasms of the muscles of the eye as force out the vitreous humour, and do such mischief that violent inflammation ensues; 7. If the pupil is large and the lens small, the vitreous humour is very liable to escape; 8. Under the opposite circumstances there is great danger; 9. In hydatid cataract the pupil is so dilated that the pressure of the operation generally forces out the vitreous humour; 10. When the vitreous humour is disorganized, it bursts forth with lens, in most cases, as soon as the cornea is divided; 11. And principally, Under the most advantageous circumstances, the iris may be wounded during the section of the cornea, which indeed cannot always be made of the proper size without difficulty; the wound of the capsule by the kisistome or some other instrument, is sometimes difficult, and may be dangerous, from spasm, &c. during its performance; the iris may protrude; the pressure of the last stage of the operation is always hazardous; 12. Many states may be undiscoverable before the operation, and yet highly unfavourable; as adhesion of the capsule to the iris, or of the lens to the inside of the capsule; morbid thickening of the capsule rendering the wound too inconsiderable; a violent contraction of the iris:—these circumstances may render a degree of pressure necessary, which is followed by unpleasant effects at the time, and violent inflammation afterwards; 13. Extraction is occasionally followed by amaurosis; 14. Inflammation often ensues upon the most favourable operations of extraction, sometimes opacity of the capsule and cornea, sloughing, &c. These are all discussed both with great knowledge of nature and ophthalmic works, and great ingenuity.

IV. *Analysis of the Author's New Operations.*— We shall first give the description of Sir W.'s operation for solid cataract in children and adults, more extensively applicable and more generally successful, he states and we believe, than any other, ancient or modern.

"Having secured the eye, by making a gentle pressure with the concave speculum, introduced under the upper eyelid, I pass the two-edged needle through the sclerotic coat, about a line behind the iris, with the flat surface parallel to that membrane; it is then carried cautiously through the posterior chamber, without in the slightest degree interfering with the cataract or its capsule. When the point has reached the temporal margin of the pupil, I direct it into the anterior chamber, and carry it on as far as the nasal margin of the pupil, in a line with the transverse diameter of the crystalline lens. I then turn the edge backwards, and with one stroke of the instrument cut in halves both the capsule and cataract. By repeated cuts in different directions, the opake lens and its capsule are divided in many pieces, and at the same time I take particular care to detach as much of the capsule as possible from its ciliary connexion. As soon as this is accomplished, I turn the instrument in the same direction as when it entered the eye, and, with its flat surface, bring forward into the anterior chamber as many of the fragments as I am able; by these means the upper part of the pupil is frequently left free from opacity. By cutting in pieces the capsule and lens at the same time, not only is capsular cataract generally prevented, but the capsule is also much more easily divided into minute portions than when its contents have been previously removed.

"The needle which I employ in this operation is eight-tenths of an inch long, the thirtieth part of an inch broad, and has a slight degree of convexity through its whole blade, in order to give it sufficient strength to penetrate the coats of the eye without bending. It is spear-pointed, with both edges made as sharp as possible, to the extent of four-tenths of an inch. Above the cutting part it gradually thickens, so as to prevent the escape of the vitreous humour."

The next operation to be considered is that for *capsular* and *adherent capsular* cataract.

"The needle used in this operation, although slightly curved, is much less bent at its point than that recommended by Professor Scarpa; on which account the surgeon can direct the point of it with greater ease and precision, for the purpose of separating minute portions of capsule, when adherent to the iris. In this operation the capsule should be lacerated very freely, as much as its consistence will possibly admit of; but when that membrane is too thickened to enable the operator to accomplish this object, he must then detach it from its ciliary connexion, except at one small part, as I have already described, and afterwards place it out of the axis of vision. In effecting these steps there is less likelihood of wounding the iris, even than in the operation for children and young persons, from the point of the needle, which is introduced at the same distance behind the iris, being somewhat curved, and directed towards the bottom of the eye, the pupil being dilated by the belladonna.

"When the capsule adheres to the iris, constituting '*adherent capsular* cataract,' the operator must proceed more cautiously; otherwise, in liberating the adhesions between the cataract and the iris, the latter membrane may be injured, and thereby give rise to severe inflammation; or should the adhesions be very firm and extensive, a forcible attempt at separation would be likely to detach the iris from the ciliary ligament, and consequently occasion an obliteration of the natural pupil. In this, as in the operation for capsular cataract without adhesions, care must be taken not entirely to detach the capsule from the ciliary processes, as it would float about in the vitreous humour, and probably obstruct the pupil; but on the other hand, if not sufficiently detached, it will be likely, particularly if much thickened, to rise again into its former situation. Should these accidents occur, the evils resulting from them may be avoided; the floating portions may be extracted through a small puncture in the cornea; and, if the capsule should rise, the operator may again introduce the needle through the same puncture in the eye, and detach it still further; or if the return should not take place for some time after the operation, it may be again repeated, and the further detachment effected, as may be necessary.

"In '*adherent lenticular cataract*,' when the pupil is not too much contracted, provided the nucleus of the lens is sufficiently soft to admit of division, I employ the same two-edged needle as in operating for the solid cataract in young persons. Part of the fragments should then be carried through the pupil, with the point of the needle, into the anterior chamber for solution and absorption. In this case, great caution is necessary on the part of the operator, both from the small size of the pupil and from the extensive adhesion of the capsule of the lens to the iris, affording a much smaller area for the action of the needle, and thereby rendering the iris more liable to be cut or punctured with the point of the instrument while dividing the lens; in effecting this division, unless the surgeon proceed with similar caution, the iris is put so much upon the stretch as either to hazard its partial detachment from the ciliary ligament, or to produce a considerable degree of inflammation. By proper care and delicacy in the size of the needle, these dangers may, however, be wholly avoided, and are to be apprehended only when an injudicious degree of force is employed with the instrument. If the pupil be too much contracted for the purposes of vision, the surgeon, instead of using the two-edged needle to cut up the cataract, should, at the first, proceed to the division of the iris, in order to form an artificial pupil, in a manner which will presently be minutely described.

"When the lens is hard and solid, and the pupil sufficiently large to admit of its free passage, I at once convey the lens forwards into the anterior chamber, with the two-edged needle, ready for extraction; but more commonly the pupil requires enlargement before this can be effected. The iris scalpel should in this case be employed at first, with which the iris should be divided transversely full two-thirds of its extent, and the lens afterwards carried through this new opening into the anterior chamber with the point of the same instrument. Should the lens be soft and even transparent, both lens and capsule ought to be cut through and divided, the capsule being always opake; which would intercept the rays of light equally as if the lens were in the same state.

"In introducing the iris scalpel at the usual distance behind

the iris, its edge should at the first be turned backwards instead of upwards or downwards; by that means the stretching of the aperture in the coats of the eye, which ensues from subsequently turning its edge backwards, as is recommended in the operation for solid cataract in young persons, and also the escape of a portion of the vitreous humour, when partially or wholly disorganized, will be prevented. It is of great importance that this escape should not take place in this particular species of case, for the coats of the eye being thereby rendered flaccid, a sufficient resistance to the action of the knife is not afforded in dividing the iris; whereas, if the operator endeavours to accomplish his object by increasing the degree of pressure upon the instrument, he will detach that membrane from its ciliary ligament, instead of dividing it. It is necessary not only to divide the iris, but either to detach or divide the capsule of the lens to the full extent of the opening of the iris, otherwise its radiated fibres cannot retract the edges of the divided membrane sufficiently distant from each other, to prevent their reunion by the first intention; unless, indeed, some portions of the fragments of the divided lens are interposed between them.

"For the purpose of dividing the iris I introduce the point of the instrument through the coats of the eye, about a line behind that membrane. The point is next brought forward through the iris somewhat more than a line from its temporal ciliary attachment, and cautiously carried through the anterior chamber, until it nearly reaches the inner edge of that membrane, when it should be drawn nearly out of the eye, making gentle pressure with the curved part of the cutting edge of the instrument against the iris, in a line with its transverse diameter. If, in the first attempt, the division of the fibres of the iris is not sufficiently extensive, the point of the knife is to be again carried forward, and similarly withdrawn, until the incision is of a proper length. I take care, however, very freely at the same time to cut the cataract in pieces. Some of these pieces I bring into the anterior chamber, and leave the remaining portions in the newly formed opening of the iris. These act as a plug in preventing its reunion by the first intention, and assist the radiated fibres in keeping the pupil

more extensively open; by the time these fragments are dissolved the iris has lost all disposition, or indeed power, of again contracting, its divided edges having by that time become callous, and being drawn considerably apart by the permanent contraction of the radiated fibres."

We must trouble our readers with one more extract upon the operation performed by Sir W. when the cataract is found too hard to be cut in pieces.

"The eye is first prepared for the operation by the application of belladonna; a very weak solution of which I employ over night. I prefer doing this in consequence of finding it advantageous, while making the opening of the cornea, that the pupil should resume its natural size as soon as the opake lens has been placed in the anterior chamber, which could not have been the case were the solution of belladonna to be made strong, and applied but a short time before the operation. The first part of the operation should be conducted precisely as if the cataract were of the soft kind, by introducing the two-edged needle through the sclerotica, a line behind the iris, with its flat surface parallel to that membrane. Its point is then to be directed through the posterior chamber, on a line with the transverse diameter of the opake lens; when its edge should be turned backwards, and a complete division of the capsule and lens be attempted, in the manner which will hereafter be more particularly described. If upon trial the lens be found too hard to admit of an immediate division, the point of the needle should be withdrawn a little, and then carried something below the line of the transverse diameter of the cataract; when, upon making pressure with its flat surface against the latter body, it becomes dislocated, and the upper part tilts forwards through the pupil into the anterior chamber; after which, without any difficulty, it may be entirely carried through the pupil, and with its posterior part turned forwards. When this is effected, the operator, with the point of the needle, taking care, however, not to wound the iris, should lacerate or cut in pieces the remaining part of the capsule throughout the whole extent of the dilated pupil, by which means secondary cataract is certainly avoided, unless an adventitious membrane be formed in consequence of inflammation. Having accom-

plished this important part of the operation, the needle is to be withdrawn, when the operator should proceed to extract the opake body.

"The patient should now be laid down on a table on his back, with the head somewhat raised, which is a far preferable position to his sitting in a chair; whereas the latter position is the best for executing the primary part of the operation,—the bringing the opake lens into the anterior chamber. The operator then makes an opening in the temporal margin of the cornea, with a lancet, or double-edged extracting knife. This opening is enlarged, both upwards and downwards, with a small curved knife, in shape and size similar to the probe-pointed knife described by Baron Wenzel, with the button removed, until it is made sufficiently large to admit of the free passage of the lens; through which a small hook is introduced, with its flat surface between the anterior part of the iris and the posterior part of the lens, which should be carried to the centre of the pupil: the curved part is then turned forwards, and the cataract laid firm hold of, when it is extracted without any difficulty."

This last operation Sir W. first performed, and with success, in June 1812. In January 1813, he repeated it upon some Greenwich pensioners, as may be seen in the hospital-books of Greenwich Hospital. This official record of the fact has turned out a favourable circumstance, for in 1814 Mr. Travers published the same operation without any allusion to Sir William Adams, adding, that he had performed it twelve months before. The Greenwich Hospital books sufficiently establish our author's entire claim to the invention, as printed or official dates must always be the test of priority. But Sir W. very sagaciously, though mildly, observes, that he happens to know, that, up to the latter end of 1812, Mr. Travers always performed the old operation of extraction at the London Eye Infirmary; and that one of the Greenwich pensioners, placed under Sir W.'s care, had been operated upon thirteen times by Mr. Travers, and had a cataract still floating in his eye.

For minute information we of course refer to the work itself, which is undoubtedly the best upon the subject, and firm-

ly establishes Sir William Adams's character as the first practitioner in cataract among us.

The proof of the superiority of the new operations invented by our author, is the astonishing success which has hitherto attended them.

Canine Pathology, or a full Description of the Diseases of Dogs; with their Causes, Symptoms, and Mode of Cure, &c. By DELABERE BLAINE, Veterinary Surgeon, and Professor of Animal Medicine in general. 8vo. pp. 234, London, 1817.

[From the London Medical Repository, for October, 1817.]

IF the nature, the economy, and the habits of the lower animals be objects of interesting inquiry to the naturalist, and their anatomy to the medical philosopher, surely a knowledge of their diseases can neither be unimportant nor unprofitable to the practitioner of the healing art. To one of the morbid affections of the dog, in particular, the attention of the profession has been forcibly impelled, by the awful consequences which result to man from the bite of the animal in that state; but hitherto the disease in the dog which produces that in the man, has been scarcely investigated: although it appears obvious, that the more rational mode of examining into its nature as it attacks man, would be first to trace it from its supposed origin in the dog. But if the examination of the nature of rabies in the dog has been overlooked, still more have the diseases generally affecting this valuable, although humble, companion and servant of our species been neglected; and even the slightest attention to them has been most improperly regarded, not only as a waste of time, but as a professional degradation. As Mr. Blaine must have been at one time susceptible of these impressions, in common with every other member of the profession, we are not surprised that he should now reflect, with "some pride and self-approbation," that he is "the first person in this country who has systematised and brought forward a [the]

regular medical treatment of the diseases of these animals, founded on a knowledge of their anatomy and animal economy;" and we are satisfied that those who peruse his work will not only exonerate him from disgrace, but yield him their thanks; and that his exertions in the cause of the brute creation will secure to him "the approbation of every humane and benevolent mind."

To justify his deviation from the regular track of his profession, and to show that, besides inclination, "the powerful operation of accidental circumstances" led to his adoption of the line of practice which he follows, Mr. Blaine has give a sketch of his professional life in the preface to his volume. We do not conceive it necessary to make any extracts from that narrative; but shall pass on to the Introduction, which contains a slight sketch of the natural history of the dog, and several very interesting anecdotes illustrative of his sagacity and high moral qualities.

Our author agrees with Buffon in supposing that all the tribe of dogs has sprung from one common stock; but, instead of supposing with that great naturalist, that the *sheep dog* was the common parent, he thinks "the species which approaches nearest to the original is the *Asiatic* or *Indian dog*, eaten by the natives." He conceives the original dog must have been intended to outstrip in speed the animals he preyed upon; hence "the sharp-pointed head, the small upright ears, deep fore quarters, with great length and strength in the hinder ones;" qualities which are eminently possessed by the Asiatic dog, who is besides "ferocious, suspicious, watchful, and sleeps lightly;" and further possesses the seeds of generosity, fidelity, and gratitude. Accidental circumstances, change of climate, and artificial causes, are sufficient, in his opinion, to account for all the varieties of form in the tribe; while most of the moral qualities of the animal are altogether the result of cultivation or domestication with man. To illustrate these qualities, many anecdotes, as we have already remarked, are detailed, either from our author's own observations, or on good authority.

Our author commences the proper subject of his work by some general remarks on the effects of remedies on dogs, and the mode of administering them, which convey several facts of

considerable importance to those who, being engaged in experimental inquiries on the effects of poisons or of medicines on the animal œconomy, choose the dog as the medium of their experiments. Thus we are informed that ten grains of calomel, which is by no means a destructive dose to a human subject, has killed a large pointer; that three drachms of aloes may be taken by some large dogs with impunity; and that it is almost impossible to poison a dog with opium, a drachm producing but little effect; while, in general, the largest dose that can be given is "returned before its narcotic influence is felt." The difficulty of administering substances to dogs has been felt by all experimentalists; and we confess the knowledge of the directions contained in the following paragraphs would frequently have saved us much trouble.

"Place the dog upright on his hind legs, between the knees of a seated person, with his back inwards (a very small dog may be taken altogether into the lap). Apply a napkin round his shoulders, bringing it forwards over the fore legs, by which they become secured from resisting. The mouth being now forced open by the pressure of the fore finger and thumb upon the lips of the upper jaw, the medicine can be conveniently introduced with the other hand, and passed sufficiently far into the throat to ensure its not being returned. The mouth must now be closed, and kept so, until the matter given is seen to pass down. When the animal is too strong to be managed by one person, another assistant is requisite to hold open the mouth; which, if the subject is very refractory, is best effected by a strong piece of tape applied behind the holders or fangs of each jaw.

" The difference of giving liquid and solid medicines is not considerable. A *ball* or *bolus* should be passed completely over the root of the tongue, and dexterously pushed some way backwards and downwards. When a *liquid remedy* is given, if the quantity is more than can be swallowed at one effort, it should be removed from the mouth between each deglutition, or the dog may be strangled. The head should also be completely secured, and a little elevated, to prevent the liquid remedy from again running out.

" Balls of a soft consistence, and those compounded of

nauseous ingredients, should be wrapped in silver or other thin paper, or they occasion so much disgust as to be returned."—p. 4.

This section contains also some excellent remarks on "the intenseness of mental feeling" in the sick dog, and the necessity of attending to its effects, which may be perused with advantage by the majority of medical practitioners. In the observations on the operation of astringents, the fact is stated, that sugar of lead (plumbi superacetas), when injected per vaginam in uterine hæmorrhages, to which bitches are liable, "often produces violent colic."

The article *Breeding* contains some curious physiological observations. The effect of the male parent on the progeny, for example, is well illustrated by the fact that bitches, by uniting in their œstrum with any dog, however large, often die in their pupping time "from the excessive size of the puppies." The appearance of the young to either of the parents, however, does not seem to be regulated by any fixed laws; for sometimes the puppies are of a mixed breed, between that of the father and of the mother, when they differ; while occasionally some are of the same breed as the one parent, and some of that of the other: thus a pointer bitch, breeding by a setter dog, will produce some pointers and some setters. We may remark that something analogous to this obtains in the human species; for, occasionally, in the family of a negroe woman, by the same white man, we see some of the children extremely dark, with features closely approximating to those of the African, while the others have the features of the father, and are nearly as fair as European children. Our author is disposed "to think that bitches are capable of superfœtation;" but he does not support his opinion by any very satisfactory reasoning.

Bronchocele is a common complaint in some particular breeds of dogs; and Mr. Blaine remarks, "the treatment is not difficult, nor unsuccessful, when early adopted." In the human species the reverse is the case; and we believe we may assert that the internal remedy he recommends, a combination of *burnt sponge* and *nitre*, has failed in the hands of most medical practitioners. We would, nevertheless, recommend a trial of his external application, which is composed of equal parts of

mild mercurial and blistering ointment, and is ordered to be rubbed upon the swelling once a day, " wrapping up the neck with a bandage, to prevent the ointment from being rubbed off:" but as an application to the human skin, it might be adviseable to take one part of the blistering ointment only to three of the mercurial.

On the subject of the *Distemper*, our author asserts, that " in England at least, hardly any dog escapes it." The attack commences at no fixed age, but generally before the dog attains his full growth, and seems to depend on a constitutional predisposition. Second attacks are not uncommon; but a third attack is extremely rare. Its symptoms resemble, in some degree, those of violent catarrh in the human species; and, like it, the occasional cause is cold; but it is also contagious, and the danger is in proportion to the youth of the animal. It is now and then attended with a particular eruption, and almost always with diarrhœa and convulsions. The treatment is, vomiting and purging in the commencement, followed by febrifuge medicines and astringents; or, if convulsions be present, by a compound of ether, laudanum, camphor, and ammonia. The fits, when present, are shortened by sprinkling cold water in the face, and by soothing language; the intenseness of the mental feeling being much augmented while dogs are labouring under this disease.

We would recommend to Mr. Blaine, the trial of inoculating dogs with vaccine lymph, as a preventive from the *distemper;* since, from the experiments of *M. Nauche* of Paris, (*See Journal de Médecine, Chirurgie et Pharmacie, Mars* 1817.) although not very satisfactorily detailed, yet they afford sufficient reason to suppose, that vaccination in many instances proved a prophylactic against the contagion of this morbid affection, so fatal to the canine race.

Dropsy, in most of its varieties of form, is by no means an uncommon complaint in dogs. It is not so frequently idiopathic as in man; but is generally the consequence of some organic affection, as, for example, old asthmas, diseased liver, and obstinate or neglected mange. Tapping has been resorted to by Mr. Blaine, but with as little permanent utility as this operation affords in man; nor have diuretics proved more suc-

cessful. Contrary to what might be expected, *hydatids* are rare in these animals.

The observations on *Feeding* and *Exercise* may be perused with advantage by those who, following sedentary occupations, indulge freely in the luxuries of the table.

Dogs are subject to both acute and chronic *Hepatitis*, as well as *Pneumonia*, *Gastritis*, and *Enteritis*. In all these complaints the practice differs but little from that which is employed in similar affections in the human animal.

The most important article in the volume, and that for which we have chiefly brought it before our readers, is that on Rabies, which, the work being of a peculiar nature, is treated of under the title " *Madness.*" The advantages our author possessed for observing this disease in all its bearings, and the morbid changes connected with it, in the system of the dog, have been, perhaps, never before enjoyed by any person; and when we also consider his capability of profiting by the opportunities he enjoyed, the facts he has brought forward become highly interesting and valuable.

Mr. Blaine justly regards the term *hydrophobia*, when applied to designate this disease in the dog, as highly exceptionable; since the animal, "instead of showing any dread of water, in most instances seeks it with avidity, and laps it incessantly." He notices the evils that this opinion has led to, in lulling into dangerous security persons bitten by dogs actually rabid; and in particular, refers to one instance, in which an eminent physician, on being consulted by a person bitten, " recommended that no precautions might be taken," because he was informed the dog could drink.

" His advice," adds our author, " had it been followed, might have caused the death of three persons. Fortunately for them, his opinion was not attended to, and I dissected the wounded parts out of each of them. In five weeks, an unfortunate spaniel, who had been bitten by this same dog, became mad; and in six weeks a horse, bitten by him, became mad also."—p. 99.

It often happens, however, that, from a paralysis of the lower jaw, the dog is unable to swallow the water which he so eagerly laps; but, even in this case, so far from betraying dread or disgust to water, Mr. Blaine remarks, ". I have fre-

quently seen the nose thrust up to the eyes into a vessel of cold water, purposely to enjoy the sense of cold occasioned thereby."

Another absurd popular error, combated by our author, is the opinion that the *worming* a dog, which is merely removing the frænum from the tongue, " will prevent his becoming rabid at any future time."

As far as the dog is concerned, the term *madness*, although adopted by Mr. Blaine in compliance with general prejudice, is regarded as improper; as in very few instances are the mental faculties much distorted, and the animal recognizes the voice of his master, and is obedient to it, frequently to the last moment of his existence.

" In other animals, however, there is more propriety in the term; for even the peaceable sheep become astonishingly ferocious in this malady. In the rabid horse, the sight is most terrific: I have seen one clear a six-stall stable of racks, mangers standings and posts; and every thing, but the bare walls, was levelled into ruins around him. On the mal-appropriation of this widely diffused term I shall have numerous other occasions of remarking."—p. 102.

Another opinion still, which our author endeavours to controvert, but certainly we think with less force, is that which supposes, that the disease is occasionally bred in the dog who exhibits it; while he maintains " that every rabid dog must have been previously bitten;" and this opinion he grounds " in long experience," and a mature consideration of the subject. Consistent with this doctrine also, he conceives it is " erroneous to suppose, that the disease is more prevalent during summer than in winter." We do not mean to attempt to weaken the force of these remarks; but we must confess we cannot shake off so completely our fostered prejudices as implicitly to adopt them. We are willing to admit, however, that " tradition and error may have implanted these prejudices in our mind in common with that of the public, and that we may find on attentive examination and from experience, that they are wholly without foundation."

Our author's description of the symptoms of rabies is not very methodical; but in our analysis of it we shall endeavour to give it a more perspicuous arrangement.

When a healthy dog is bitten by another which is rabid, the disease seldom shows itself before the third or fourth week after the accident; sometimes the seventh week passes before the symptoms are apparent; and occasionally three or four or more months elapse. Accidental circumstances, however, hasten the attack; as, for example, a long and fatiguing journey; a cold; or, if the animal be a bitch, the presence of the œstrum. Something also depends on the part that is lacerated; for when the head has been bitten, the symptoms of rabies sooner appear. The first symptoms are very obscure, and consist chiefly in some peculiarity of manner in the animal; as, for instance, the picking up and swallowing of straws, threads, paper, and many other things that a dog will not eat when he is well: he gnaws also every article within his reach; there is a great predilection for applying the nose to any thing that is cold; and he frequently scratches and bites himself, particularly in the part which was bitten. The appetite is not much impaired, although some cases are attended with sickness; but the taste is often so depraved, that the animal eats his excrement and laps his urine. After a day or two, the dog generally leaves home and roves about, displaying a great antipathy to other animals, particularly his own tribe, and biting every dog that comes near him. He seldom however attempts to bite a man, unless hunted into fury, or otherwise much irritated; but occasionally very little irritates him, such as the merely presenting to him a stick or the foot. The usual meekness of the animal is often conspicuous to the last, particularly to his master; but sometimes, whilst every mark of tractability is displayed, "he will be very likely to turn on a sudden, and snap at the person who is caressing him." When roving about, some dogs are silent; but the majority howl or bark with a very altered tone of voice, with is readily distinguished from that of health. If an affected dog come up to another "little or large, he goes up and smells to him, in the usual way of dogs, and then immediately falls on him, usually giving him one shake only: after which, he commonly trots off again in search of another object. The quickness with which this attack is made, very frequently surprises the bitten dog so much, as to prevent his immediately resenting it: but nothing is more erroneous than the supposition, that a healthy dog instinctively knows a rabid or mad one. I have

watched these attacks in numerous cases, and I have seen the mad dog tumbled over and over, without the least hesitation, by others that he had attacked."—p. 113.

The younger the dog is, the greater is the disposition to rove, the more striking are the symptoms of mental alienation, and he is proportionally wilder and more mischievous. These are the usual symptoms of the disease in the dog; but scarcely two cases are alike in any of the particulars. Our author ascribes this diversity to the parts principally attacked.

"When the inflammation exists most in the bowels, it generally produces an affection of the neck and throat. This affection consists in a trifling enlargement of all the parts around; the tongue hangs out, and is discoloured, and, from a partial paralysis of the muscles of deglutition, there is frequently a difficulty, but never any disinclination to swallowing. In such cases there are also greater heaviness, stupor, and distress, with a marked weakness of the hinder parts. It is this kind, from the dropping of the jaw, that sportsmen are led to call dumb madness; but it is evidently incorrect so to call it, because it is often accompanied with howling.

"On the contrary, when the lungs are the principal seat of the affection, there are usually much more quickness and irritability in the dog's manner. He rather barks, with a hasty and altered tone, than howls. He snaps at passing objects, as flies, and shakes his chain, or the vessels he drinks out of, with seeming violence: but all this is the irritability of a moment; for the voice of his owner will generally quiet him at once. As in some cases, however, both the abdominal and thoracic viscera are nearly equal partakers of the specific inflammation; so these symptomatic appearances are often blended, and appear, though in mitigated degrees, in the same subject."—p. 110.

The eyes are commonly red and suffused in the early stage of the disease, and now and then ulceration of the pupil supervenes. The animal often displays symptoms of uneasiness in the bowels, by gathering straws and applying them to the bowels; and costiveness is always present.

Sportsmen notice two varieties of rabies, which they denominate *dumb* and *raging*. The former is the more common;

and is usually attended with a considerable affection of the mouth and throat.

"To speak anatomically, the whole of the pharynx and larynx are tumified, and the surrounding muscles affected with paralysis. From this cause the lower jaw drops, and is incapable of remaining closely applied to the upper. The tongue becomes also affected with the paralysis, and hangs pendulous without the mouth. A similar torpor apparently pervades the blood vessels of these parts, which become filled with venous blood: the tongue in particular is black, more especially so at the apex or point: sometimes a black stripe extends along the whole extent. The paralysis extends to the back of the œsophagus, from which a great difficulty is frequently experienced in swallowing; but in no instance is any dread of liquids expressed; nor does even the attempt to swallow appear to give pain. The larynx, participating in the affection, occasions a deep choaking kind of noise, which seems to issue from the bottom of the glottis.

"Sometimes the mouth is quite dry and parched; at others it is very moist, and a quantity of saliva continually flows from the jaws. When the mouth is affected in this manner, the sufferings of the poor animal are extreme, for his thirst impels him to be continually lapping; but the paralysis of the lower jaw prevents his retaining the liquid in his mouth, and it falls out as fast as taken in."—p. 115.

The duration of the disease varies very considerably. It seldom proves fatal before the third day; but few dogs survive the seventh. The average deaths happen on the fourth and fifth days.

Our author had many opportunities of ascertaining the morbid appearances presented by dissection in dogs affected with this disease, and he has availed himself of them. When there has been much irritability, panting, and disposition to mischief, the brain and its membranes exhibit marks of increased vascularity; and the same is the case with the lungs, one side of which is generally more inflamed than the other; and when the animal has actually died of the disease, the lobes of this viscus are found black and gangrenous. When however there has been a disposition to collect straws, and to place them under

the belly, and to swallow indigestible substances, as hay, rope, wood, coals, &c. the stomach and bowels chiefly are inflamed, and the above enumerated substances are found united, and forming a crude mass in the stomach. This is regarded by our author as one of the most certain evidences of the previous existence of the disease. The mesentery, the liver, and the diaphragm, also occasionally exhibit marks of inflammation; but this Mr. Blaine regards as altogether sympathetic. In the majority of cases, some inflammatory appearances are visible at the posterior part of the mouth, and in the pharynx; and a peculiar inflamed spot, which is highly characteristic of the disease, is always discovered at the back of the fauces.

Mr. Blaine very justly remarks, that neither in the dog nor in the human animal ought we to place much reliance on any method of preventing the disease after the bite has been inflicted, except the part be excised. This can always be effected in man; but in animals it is sometimes difficult to discover the parts that have been bitten; and consequently he was desirous to discover some preventive of the attack; and he is fully persuaded he has accomplished this in the following recipe, which he obtained from "a cottager of the name of Webb, near Watford." We subjoin it, not because we would place much reliance on its efficacy, but that it may be submitted to a fair trial on dogs; for we should be sorry to find the safety of any of our fellow creatures confided to its powers.

"The following, which is an improvement on the original formula, is that which, after much experiment, I find the best method of preparing the remedy:—

Take of the fresh leaves of the tree box...2 ounces
Of the fresh leaves of rue.....2 ounces
Of sage.....half an ounce.

Chop these finely, and, after boiling them in a pint of water to half a pint, strain and press out the liquor. Beat them in a mortar, or otherwise bruise them thoroughly, and boil them again, in a pint of new milk, to half a pint, which press out as before. After this, mix both the boiled liquors, which will form three doses for a human subject. Double this quantity is proper for a horse or cow; two thirds of it is sufficient for a large dog, calf, sheep or hog; half of the quantity is required for a middling

sized dog; and one-third for a small one. These three doses are said to be sufficient, and are directed to be given, one of them every morning fasting. Both the human and brute subject are treated in the same manner, according to the proportions directed.

" In the human subject I have never found it produce any effects whatever but a momentary nausea from disgust. To prevent this disgust operating disadvantageously, the old recipe directs it to be given two or three hours before rising; which is not a bad plan, because it will be less likely to be brought up again by such precaution, which so large and unpleasant a dose might otherwise be. Neither in any animal, except the dog, have I ever witnessed any violent effects from the exhibition of this remedy. In dogs, however, I have frequently seen it produce extreme nausea, panting and distress; in two or three it has even proved fatal: but, as I conceive that it is more likely to be efficacious, when it shows its effects on the constitution; and as, at the same time, it is proper to guard against these effects being too violent: so it is prudent always to begin with a smaller dose than the one prescribed, and to increase it each morning till it shows its activity, by sickness of the stomach, panting, and evident uneasiness. In such cases, perhaps five doses are not too much.

" In a long and successful practice, I have given this remedy to nearly three hundred living beings. About fifty human persons have taken it, eight or nine horses, several sheep, and a few cows and hogs. The rest were dogs; but in almost all I was enabled to trace the history of the danger, to a conviction, that the animal concerned had been bitten by a dog unquestionably mad. Out of this number, I am happy to state, and which I conscientiously and solemnly do, that only nine or ten instances of failure have occurred; but candour obliges me to own, that four or five of these were palpable and fair cases; for the medicine was given *apparently* with every caution."—p. 128.

Our author nevertheless leans as he ought to do to the superior safety of excision; but he thinks " it is of no consequence that the excision or cauterization should be immediately effected; as it is equally efficacious if done at any time previous to the secondary inflammation of the part bitten. We would accord

with this opinion, did we conceive it likely that individuals would as readily submit to excision after some time has elapsed from the infliction of the bite, as directly after it has been inflicted; but as this is not to be expected, we are of opinion that the excision cannot be too soon effected; although it ought not to be left undone, should even some days have passed, under the impression that it is then too late.*

The only notice we conceive necessary to be taken by us regarding *mange*, which is the subject of the next section, is to mention the idea of our author, that "the canine mange is capable of producing the human itch." We are not prepared to deny the assertion, but we doubt its accuracy; for as very few dogs escape an attack of mange, were it capable of communicating scabies to the human subject, we should find the disease more prevalent than it is, particularly among the lower classes of people, among whom, few families are without a dog.

Rheumatism is a disease almost as common to the dog as to man; but in the quadruped it is attended by some peculiarities. Thus in the dog, it is never present without affecting the bowels; and paralysis of the hind legs is also a common accompaniment of the complaint, when it affects the lumbar muscles. The treatment recommended by our author, consists of the internal exhibition of purgatives combined with opium, the use of the warm bath, and the local application of an embrocation composed of oil of turpentine, spirit of hartshorn, laudanum, and sweet oil, in equal proportions.

Dogs are very subject to tænia, the lumbricus teres, and ascarides: they are also troubled with another kind of worm which is not found in the human body, and which Mr. Blaine describes very vaguely, by saying that it has a short body resembling that of a maggot, and a red or black head. There is nothing new in the treatment recommended; but we are surprised that he appears to be ignorant of the effects of oil of turpentine in tænia.

We conceive it unnecessary for us to go though the remaining articles of the volume, which are of a nature that renders them interesting to those only who are immediately concerned in the treatment of canine diseases.

* For our particular opinions on this subject, see *Repository*, vol. iii. p. 47-54.

In concluding, we have only to remark, that in a general point of view, this volume is certainly interesting, as extending the limits of pathology, and affording an illustration of the influence of morbid causes on the animal frame, modified by habit and circumstances. Yet reflecting that our author received a regular medical education, we have been struck with the limited knowledge of physiology it displays; and the few efforts he appears to have made to illuminate many important questions in the doctrines of animal life, compared with the many opportunities he possessed. In a second edition of the work, he may supply this deficiency; and we would also recommend him to improve his style of writing, and to correct the many grammatical inaccuracies that deform the pages of the present.

Observations relative to the Use of Belladonna in painful Disorders of the Head and Face; illustrated by many Cases. By John Bailey, Medical Practitioner, of Harwich.

[From the Annals of Medicine and Surgery, for December, 1817.]

No one is unacquainted with Dr. Marcet's interesting paper on the remarkable anodyne properties of the extract of stramonium, inserted in the sixth volume of the Medico-Chirurgical Transactions, and reviewed in our 5th Number. As we have given a very extensive trial of this medicine, we shall take this opportunity of giving our testimony to its virtues.

We have always employed the extract prepared at Apothecaries' Hall, or Hudson's in the Haymarket. Dr. Marcet recommends beginning with an eighth of a grain, and gradually increasing the dose, if requisite, to one grain or more. In no instance, however, has an eighth of a grain proved at all active with us; and we therefore now commence with one third, and if the pain is violent, with one half a grain, increasing the dose by one third, or one half every day, till the object is attained. Half, or a whole grain, repeated as Dr. Marcet directs, gener-

ally proves sufficient; but we have exhibited it in doses of two, and in one instance, in doses of between five and six grains several times a day. This instance was a case of uterine disease, and the patient found the medicine invaluable; for she had it always in her power to remove or diminish the pain, and without any of those effects which had rendered it impossible for her to persist in the use of opium: in fact, no circumstance accompanied its anodyne effects, except a dryness and heat of the fauces. We have never once found those disagreeable effects which are common to the employment of narcotics, and on account of which some practitioners have spoken against stramonium; such as vertigo, temporary amaurosis, &c. In a case of spasm of the præcordia, we lately gave a young woman, who had not been, like the patient with uterine disease, gradually accustomed to its exhibition, a grain every hour, for eight hours; and to another, suddenly seized with spasm of the diaphragm, three grains in a quarter of an hour, without any unpleasant effect: but as the medicine proved totally inert, from the great violence of the disease in each case, these are scarcely fair instances. It may be observed, by the way, that as a very large dose of some powerful narcotic was deemed requisite in each of these cases, we fixed our reliance upon opium, as being at present better acquainted with it, and gave at once an immense dose with success. In diseases, whose grand feature is pain, we have never failed with stramonium, either to cure, or remarkably palliate. In all cases of neuralgia or tic douleureux, even of the most excruciating kind; and in rheumatic head-achs, face-achs, and ear-achs, in chronic rheumatism without swelling, and in wandering pains, our success has been astonishing. In painful inflammatory diseases, we find it much superior to opium, as an adjunct to calomel and antimony. In those distressing cases where medicine and surgery are unvailing, and opium is esteemed the only solace, we have no hesitation in asserting stramonium to be highly superior: with us it has induced no feverishness, no stupor, and, what is of still more consequence to the patient's comfort, no continued and obstinate constipation. Dr. Scudamore has found stramonium in many cases not very effectual, and asserts the superior advantages of combining

lactucarium with it. We suspect he has not given it in sufficient quantity, and that an increased dose would have answered much better than the addition of lactucarium, which is a comparatively inert drug, and has been relinquished at Guy's Hospital, on account of its inertness.

Exactly what Dr. Marcet's paper was in regard to stramonium, is Mr. Bailey's in regard to belladonna. He proves its remarkable property of relieving pain, by the narration of numerous cases; but gave it in such doses as often to induce very unpleasant, although temporary, symptoms; such as amaurosis, swelling and protrusion of the tongue, dryness of the throat, great thirst, suffusion of the eyes, &c. Perhaps these effects might be avoided without impairment of the benefit, by exhibiting it in small and repeated doses, as Dr. Marcet does with stramonium, instead of large doses at long intervals, as Mr. Bailey recommends. The relief in almost every instance was surprising; a cure was effected in many. A few, for the most part accompanied by inflammation and pyrexia, were not benefited, but we have reason to think that the addition of calomel and antimony would have rendered the result different.

We see no necessity for transcribing any of the cases, which were all of a nature well known to every practitioner: suffice it to say, that Mr. Bailey recommends either the Extract of the London Pharmacopœia, in doses of three grains, or from twenty to forty drops of a tincture made by steeping two ounces of the dried leaves in a pint of spirit.

Some cases, also, are related, which show the powers of belladonna over the same diseases in the hands of other practitioners. In a violent example of hysteria, which had withstood all other remedies, its efficacy was indisputable.

Its medical virtues appear very analogous to those of stramonium, and to one or other, or both, we hope many affections, hitherto often irremediable, will now generally yield. We trust that a fair trial will be made of both medicines, in various affections. Their use is by no means new, but their specific powers have not been at all accurately ascertained.

We regret that in giving so interesting a little work to the public, Mr. Bailey has not confined himself to the simple narration of cases, but prefixed many very unnecessary pages.

General practitioners, actively engaged, are not supposed to be generally men of deep reading or cultivated taste; and we must always be grateful to them, and perfectly contented, when they favour us with plain practical novelties. If they attempt more than this, they do but too often spoil the good they give us; and accordingly, Mr. Bailey, to whom we return our best thanks for his important information, really put us, when reading the first quarter of his book, to more pain than all the belladonna upon earth would remove; he, in truth, apologizes repeatedly for his plain learning.

An Account of some Experiments made with the Vapour of Boiling Tar in the Cure of Pulmonary Consumption. By Alexander Crichton, M.D. F.R.S. Physician in Ordinary to their Imperial Majesties the Emperor and Dowager-Empress of Russia; Physician in Chief of the Civil Department of the Empire; Knight of the Order of St. Vladimir; Honorary Member of the Imperial Academy of Sciences of St. Petersburgh, &c. &c. &c. 8vo. pp. 62. Edinburgh, 1817. Published in French at St. Petersburgh last summer.

[From the Annals of Medicine and Surgery, for December, 1817.]

Be not alarmed, gentle reader! be not flurried! your English feelings are not going to be insulted by the offer of instruction from a Tartar. The present pamphlet comes, indeed, from the kingdom of the Huns—the dirty Huns "who stew their meat"—in short, who are mentioned in Hudibras, and whose nasty tricks are so lusciously described by Dr. Clarke. But the writer is one of us, a native of Scotland, and a graduate of Edinburgh, simply settled in Petersburgh, and he dedicates his work to Dr. Baillie. Those, therefore, whose wit might have contrived a pun upon tar being recommended by a Tar-tar will be disappointed.

Dr. Crichton confesses his acquaintance with a treatise written above thirty years ago by a Mr. Mudge of Plymouth,

entitled "A Radical and Expeditious Cure for a recent Catarrhous Cough;" the chief object of which is to recommend an inhaler in catarrhal complaints, but in which a case of violent pulmonary suppuration is related, cured by fumigation with common resin twice a day and a residence in the country. Mr. Mudge is uncertain whether the cure could be attributed to the fumigation, which he declares he never found successful in any other case; adding, however, his belief that the principal advantage of sea-voyages to consumptive patients depends upon the tar-vapour constantly present on shipboard.

The immediate cause, however, of the suggestion to Dr. Crichton, is thus detailed :

"In the neighbourhood of a country house which I inhabited last summer, in the vicinity of the Imperial Palace of Kamenoi Ostroff, is a cable manufactory, which I one day went to see, without any other motive than that of taking a walk. In the place where the tar was boiled in large quantity, I found, to my great surprise, that although the vapour affected the eyes painfully, yet I breathed the air with perfect ease. In a kind of magazine adjoining to this, where the vapour and odour of boiling tar were weaker, yet strong, the eyes were not affected; and it immediately struck me, that this artificial atmosphere might be of use to a consumptive patient who lived in the neighbourhood, and whom I had entirely despaired of curing. The proprietor of the cable manufactory immediately consented to the trial being made, and the following results ensued from it."

The patient was thirty-one years of age, with a flattish chest, a delicate constitution, and a pale complexion. He had laboured under inflammatory symptoms of the chest, cough, hæmoptoe, &c. two years; and at this period had a frequent cough, oppression of breathing, very copious purulent expectoration, and colliquative sweats and diarrhœa, with extreme emaciation. He was sinking rapidly.

"The first day he remained in the tar vapour four hours; and in a very short time after entering the room, he says he experienced a sensation of greater ease on his chest. As the weather was hot, and he had no one there to converse with,

he fell asleep, but soon awoke with a head-ach; still the relief in his breathing was so decided, that he resolved to return to the room on the morrow. He continued daily to visit it, always falling asleep, and waking with head-ach. It was not till the expiration of some weeks that the tar vapour ceased to have this effect. During this time, however, the cough and expectoration had gradually diminished, and at the end of a month he had regained his former strength; when, without informing me, he went daily to town, neglected his remedy, and the cough returned. I reproached him for such imprudence, and insisted on his having recourse again to the tar vapour room, which had before been so successful. He returned to it, and continued his daily visits there until the end of one short summer. The begining of September, the period of quitting the country, he felt perfectly well, and thought his cure complete. I then lost sight of this patient; and it is only to-day, the 27th March, 1817, that I learn, in answer to a letter I addressed to him, that his health has continued good; that he has taken no medicines all the winter; that he has gone out daily on business; but that having very lately a slight return of cough, he has just begun to cause his bed-room to be fumigated at night with the tar."

The efficacy of no medicine could be more decided than in this case, and the next is equally satisfactory.

The subject was a woman, thirty years of age, rather flat chested, thin, and of a livid complexion. For a twelvemonth she had had inflammatory symptoms in the chest; and when she consulted Dr. Crichton she was greatly emaciated, had a constant cough, an abundant expectoration of greenish-yellow matter in every point resembling pus, colliquative sweats, and diarrhœa. No hopes were entertained of her recovery; but a decoction of althæa with a little tincture of opium was prescribed, and the fumigation of her room with tar vapour.

" At first the vapour occasioned head-ach as in the former case; but as she felt considerable relief in her chest, she persisted in the trial of this remedy. She assured me, that without using any other means of cure than those above stated, she found, at the expiration of a week, that her cough and expectoration were much diminished, but that her great weak-

ness prevented her quitting her bed. Her perspiration was also lessened; the diarrhœa had ceased since her use of the decoction of althæa and opium.

Her appetite and strength soon returned, while her cough and expectoration became daily less and less. At length she rose from her bed, and sat up several hours in an arm-chair. From this period her convalescence went on rapidly; all the bad symptoms disappeared, and she regained flesh. Her cough totally left her by the time of my returning into town early in September. In a week afterwards, I heard that she had resumed her formed occupations, and felt herself perfectly recovered. From this time, until the 11th of January, 1817, I had heard nothing of her. She then came to tell me she had continued quite well the four last months of the year; that since September she had neither taken medicine nor employed the tar fumigation, but that she now thought she had again caught cold from the dampness of her habitation; that for the last fortnight her former cough and expectoration had returned, and that she already felt much weakened. I prescribed exactly the same treatment as before, and in the month afterwards, I heard with infinite satisfaction from her husband, that she was again recovered."

These remarkable cases encouraged our author to make further trials; and he therefore begged permission of her Imperial Majesty, the Empress Dowager, to pursue these trials in the Hospital for the poor which is under her Imperial Majesty's immediate protection.

The favour was instantly granted, and one ward, containing four or five beds, was appropriated for the trials, which were to be made by two of the hospital physicians, Messrs. Bluher and Roos.

Their report begins by stating, that the easiest and best mode of fumigation is to put the tar in an earthen vessel over a lamp or heated iron, so as to cause a slow volatilization, till the air of the ward is sufficiently impregnated. The process was repeated three or four times a day.

The cases in the report are six, and we shall give a sketch of each, previously advising such of our readers as have not strong teeth, not to attempt pronouncing the names of the patients, but to pass on to the history.

1. Lukeria Niketena, a young widow, ill above a year, had pains in the side, frequent cough, dyspnœa, a hectic, purulent expectoration, and night sweats. The tar fumigations produced, in less than a week, a great diminution of the cough and expectoration, and a remarkable relief in the chest. Her health rapidly improved, and on the twenty-first day, the sweats had ceased; she found herself very comfortable in the fumigated room, but on quitting it or passing into a colder air, she felt her lungs irritated, and coughing was excited. On the twenty-seventh day, the state of her chest seemed good, her cough was trifling, and the report adds, that she had often asked for dismission, saying she felt herself well, and strong enough to return home. But she was detained for further observation. After three months, she was dismissed perfectly cured.

2. Anna Woladkowtschina, a widow, aged forty, was reduced to great weakness by repeated attacks of hæmoptoe, had a cough, yellowish green purulent expectoration, mixed with mucus, hectic and colliquative sweats, and was hastening quickly to her end. In two days the cough and expectoration were diminished; and in a month she had no symptoms of disease. She made the same remark as all the others, that she felt much easier in the tar room than elsewhere; and that changes of the weather affected her chest. During two days of dry frosty weather she felt an oppression at her chest, with violent palpitation, the common precursors with her of hæmoptoe, but they subsided without this consequence. After omitting the fumigation, however, the precursory symptoms, hæmoptoe and purulent expectoration, returned, but were banished by the fumigation: and again, on intermitting the remedy, which some repairs in the house prevented from being continued a proper length of time, they returned, and again were put to flight; so that at the period of the report she had only a little cough and mucous expectoration of good quality.

3. Anna Gourtschinskaia, a widow, also aged forty, had purulent expectoration, rigors, hectic, sweats, and œdema of the legs. Her casewas hopeless, and she died very dropsical; but the fumigation, during the first five days, decidedly relieved the oppression, cough and expectoration, and prevented the pain of the chest from ever returning. As nothing could be imagined of use, the remedy was given up in five days more.

4. Maria Romanova, a widow, twenty-nine years of age,

after repeated symptoms of inflamation of the chest for four months, became the subject of purulent expectoration, cough, night-sweats, and all the attendants of phthisis. On the day after she was placed in the fumigation ward, she felt considerably eased; on the sixth day she was no longer hoarse, had no pain in the chest, and much less night-sweats. Her symptoms were much affected by the weather; but at the end of a month from the first employment of the tar she was convalescent. In another month the pain in her chest recurred, and she again recovered; but on being compelled to leave the tar-ward, for the accidental reasons above stated, she had a third relapse, and sunk.

5. Martha Ivanova, a widow, aged forty, had a chronic cough, which eventually became very violent, and was attended by purulent expectoration.

" In this situation she was sent to the fumigated ward, and immediately began to find relief. She slept the first night far better than she had done for a long time past. The following night she also slept well; the pains in her chest totally left her."

The medicines she was taking before her admission into this ward were continued; and we may here remark, that most of the patients took some medicines, which were but trifling; and however they might sometimes palliate symptoms, as an occasional blister, could clearly have no share in the cure. In a short time her cough became less frequent, and her expectoration easy, and her fever and sweats entirely ceased. But she at length grew worse and died.

6. Catherine Cornadi, twenty-seven years of age, had been ill six months, after getting wet through, with amenorrhœa, pain in the side, fever, cough, and finally with expectoration and sweats. The day after she was placed in the fumigation ward, " she said she already felt better, and had passed a better night than she had done since the commencement of her illness, and particularly that she felt much refreshed; the cough was less violent, and the expectoration less abundant. In a few hours after entering this ward, she perceived a general perspiration over her whole body. In three days more, there was a greater diminution of the cough, expectoration, and also of the perspiration. She slept extremely well, she had vomited (she before vomited every thing she took) only once, and this seems to have proceeded from some improper food, which she

had procured in a secret manner. She felt herself so well, that she begged for her dismission. Afterwards, she became less disposed to spitting of blood. The expectoration, which at first had every character of pus, became gradually better in quality, and less abundant. The sweats, cough, and vomiting, also ceased by degrees, and her catamenia returned. Feeling herself well, she incessantly urged us to dismiss her; she was sent out of the hospital the 6th of March, (ten days from the first employment of the tar) perfectly cured. We have seen her since that time, working as before her illness, and continuing in perfect health."

His excellency Dr. Crichton had also directed Dr. Wochler, one of the physicians of the Aboukoff, or city hospital, to make trial of the remedy. One small room only could be procured, and into it no more than three beds could be crammed, so that the air was quickly corrupted. But one most decidedly consumptive patient was completely cured; the other, on whom the experiment was made, continued in so fluctuating a state, that Dr. Wochler had not, at the period of publication, thought right to draw up a report of his case. The former was that of Alexander Nimaeff, aged twenty-one, of a scrofulous habit, and hereditarily disposed to phthisis. From long and fatiguing journies in cold climates, he had laboured under frequent attacks of hæmoptysis, and when admitted, was in the last stage of consumption; had violent fever, profuse and offensive sweats, vomiting, and diarrhœa; a cough, which compelled him to sit upright all night, and a copious expectoration of fœtid pus. He had been but a short time in the tar vapour when he said he was easier, and towards ten o'clock his cough grew less, and he fell asleep, nor did he awake till morning. He then said he felt better. In two months he was perfectly well, and shortly afterwards dismissed cured.

These are all the detailed cases in which the remedy has been employed. The pamphlet now closes with some remarks.

The tar fumigation gave great relief to every patient, and although it did not cure them all, this would have been almost more than could be expected, were it even a specific, when we recollect the disadvantages attending the exhibition, from the smallness of the ward, which prevented the air from being any thing but most corrupt, as the opening of a door or window

would have exposed the patients to considerable risk. In great disorganization a cure is out of the question, and could the ulcers be healed, there might not be sufficient of the organs left for the necessary functions. Dr. C. conceives that in large vomicæ after peripneumony or hæmoptysis, the great mischief which has taken place, and the impossibility of the fumes penetrating through the large body of pus to the ulcerated surface, renders the change of success less than in tubercular cases. We are not quite clear that this is the true explanation of the fatality of the three unsuccessful cases—one was evidently beyond all means. The tar cannot be expected to remove or prevent tubercles, although it may heal them when suppurated; but this is almost every thing.

To ensure purity of atmosphere, Dr. C. recommends two fumigated rooms for consumptive patients; one for sleeping, the other for the day.

When the cough and hectic are much subdued, he dissuades us from persisting in the remedy, as it frequently induces a dry cough, and prevents the enjoyment of what is then fit for the patient—common air. In one instance of extraordinary success from the tar, its use was suspended occasionally from the irritation it produced, but during the suspension the disease always became aggravated.

The tar used for the cordage of ships is found infinitely the best; and the simplest and most effectual mode of fumigating a room, is to place the vessel containing the tar over a spirit lamp, taking care that it should boil slowly and not burn. The vessel should be well cleaned every day, and the fumigation repeated every three hours.

As we have all but too many opportunities of putting this plan to the test, we trust it will immediately have a full and impartial trial, that it may be speedily either established as good, or added to the list published by Dr. Roberts, in the Transactions of the College of Physicians, of remedies useless in phthisis, although neither given in small quantity nor hastily abandoned.

We have already begun to ascertain its merits. Nothing appears, *à priori*, more rational, than to employ applications to ulcers of the lungs, as well as to external sores; and tar, as a local remedy, has been long established.

BIOGRAPHY.

Biographical Notice of Dr. Alexander Monro of Edinburgh.

[From the Edinburgh Courant, for January 1818.]

Among the many celebrated names which have adorned the University and Medical School of Edinburgh, few are more remarkable,or more justly entitled to respect, than that of the worthy and venerable character whose life we now presume to delineate.

As an able, active, and meritorious professor of anatomy and surgery, the late Dr. Monro was, for more than half a century, at the head of the great medical school of Edinburgh —and for the greater part of that time, as a practical physician, he was unquestionably at the head of his profession in Edinburgh and in Scotland, to many even distant parts of which he was often called, and from every part of which, as well as of England and Ireland, he was frequently consulted by letter in cases of peculiar difficulty and danger.

This distinguished professor was the son of another very eminent Dr. Alexander Monro, professor of anatomy in the University of Edinburgh, and father of the Edinburgh medical school; he was born in the year 1733, was from his earliest youth carefully instructed by his father in anatomy, and soon acquired such a taste for that science, and prosecuted the study of it with such ardour and preseverance, that in the year 1753, when he was just 20 years of of age, he assisted his father in his anatomical lectures; the following year was appointed conjunct professor, and his father, in 1758, entirely resigned the office to his son, who, it must be acknowledged from the father's great popularity as a teacher, had an arduous task to perform, but which his talents, upon trial, were found well calculated to support. His clearness of demonstration, acuteness of distinction, accuracy of reasoning, and depth of knowledge, soon gained him the applause and confidence of his students, and not only fully evinced how completely he was master of his

subject, but also that he possessed, in no common degree, another talent no less necessary for a public teacher—the proper mode of communicating his knowledge to others.

His lectures were universally allowed to be conducted on a plan of the most extensive utility; and it may well be doubted whether so useful a preliminary and preparative for the study and practice of physic and surgery ever had been given before, in the form of lectures, or in any other form. Accordingly, they were attended by vast numbers of students from all parts of Europe, amounting, during the period he continued to teach anatomy, to upwards of 14,000.

Dr. Monro's acknowledged merit, as an anatomist and teacher of surgery, the quickness and clearness of his perceptions in those matters, his decisive judgment, and the intimate knowledge of every part of the practice of physic, which he uniformly displayed on these occasions, soon and most deservedly procured him general confidence, and very extensive practice as a physician.

Perhaps no man of the medical profession ever more strongly illustrated, by his conduct, and by the general tenor of his practice, the important truth, that the most valuable part of a physician's merit is good common sense, steadily employed on a particular subject. It is recorded of an ancient Greek physician, whose name (Trophilus) and whose apophthegm, but none of whose writings, have descended to us, that, when he was asked, "who would be a perfect physician?" he answered, "he who is able to distinguish what can be done and what cannot be done." This merit Dr. Monro possessed in a very high degree; his just notions of general science, his thorough knowledge of medical science, especially of morbid anatomy, but most chiefly his own good sense and constant habit of strict attention and accurate observation in his practice, enabled him to perceive at once the futility of many hypothetical doctrines, with respect to the nature and causes of diseases, and led him to distrust and reject many supposed remedies and modes of practice, which might be employed in consequence of these hypothetical dogmas.

The same accomplishments equally prompted him, and enabled him to appreciate the real merits of many remedies, which,

from time to time were introduced into practice with the most extravagant applause, purely on empirical principles. Thus, most effectually preserved from two of the worst and most frequent causes of bad practice, his had the merit of being simple, rational, and powerful, and consequently as successful as can be employed in the present state of medical science.

Of his merit as a practical physician, when acting singly, his brethren cannot fail to have a high and just notion, when they recollect what his conduct was in consultation; without any subtle disquisitions, without any controversies about obscure and disputed points, without credulity as to the virtues of particular remedies, and far above the miserable vanity of arrogating to himself any superior skill, or pretending to extraordinary success in his own practice, he was generally the first to propose, and always was ready, most candidly, to agree with others who proposed those simple and powerful modes of practice, from which alone, in urgent and dangerous cases, any essential benefit can be derived.

As an author, he has given to the world many important works, and many most useful and ingenious discoveries.

His love of science was sincere and ardent, his learning was extensive, and his judgment singularly correct. His profession was the study and delight of his life, and was prosecuted with unabated zeal, at a period when the enthusiasm of youth generally subsides. Yet, though his exertions in the improvement of knowledge were at all times carried on with the most unwearied assiduity, he never allowed them to interfere with the more immediate concerns of life. He was an affectionate husband, an indulgent parent, a kind friend; cheerful and ingenuous in his temper, animated in conversation, engaging in his manners, conciliating and benevolent in his disposition; ever anxious to promote the welfare of those around him, and ever ready to contribute to the happiness, or to alleviate the distress of his fellow creatures.

At length, after a period of 80 years, the greater part of which he had devoted to the service of mankind, commanding the love and veneration of those connected with him by the dearest ties—admired and respected by the world as a

man of talents and integrity—enjoying from early youth to extreme old age almost uniform and unexampled prosperity—it was his lot to combat the pain and affliction of a protracted disease, which terminated his long and well spent life, in the 85th year of his age. Yet during his confinement and gradual increase of feebleness, and with the discouraging prospect of still greater sufferings, his patience was ever most exemplary; nor did the distressing infirmities, inseparably attendant on excessive debility, ever produce a murmur of complaint, or even a hasty expression. He had fully tasted the enjoyments of life: he had run his long career with a conscience void of reproach, and prepared for death as an event which he neither desired nor feared—thankful for the blessings he had been permitted to enjoy—resigned to the sufferings he was destined to bear—and reposing with humble confidence on the mercy of Him by whom they were inflicted.

ORIGINAL PAPER.

Observations on Mr. Charles Bell's Paper on the Muscularity of the Uterus, originally inserted in the London Medico-Chirurgical Transactions. for 1813. By WILLIAM P. DEWEES, M. D.

IN the 5th Vol. of the Eclectic Repertory, p. 37, is a paper on the muscularity of the uterus written by Mr. Charles Bell, on which we beg to make some remarks. If we merely regard the anatomical detail it contains, we have but little more censure to bestow, than might with propriety attach to any attempt that has hitherto been made to develope the muscular structure of this all important viscus; but not so with respect to the physiological and pathological inferences deduced from it. We cannot feel that Mr. Bell has been more successful in his description than Vesalius, Malpighi, Ruysch or Hunter have been—for it is confessed that these gentlemen bestowed much patient labour and industry in the investigation of this part, but without producing entire conviction on the mind of any one, that either of them was correct; for, each differs from the other in relating what he believed he saw, and what constituted the structure of the uterus. When then, men of such unquestionable talents as anatomists do not agree in their observations, we think we have a legitimate cause to doubt the *opinions* of any other on this subject, until his view shall be confirmed by repeated and carefully made dissections. "We can easily believe what we wish to be true," is an old truism, and strictly applicable to all, perhaps, who have attempted to demonstrate the muscular structure of the uterus: each appears to have had a preconceived theory to support, and his labour has been expended with a hope or perhaps a determination of finding the anatomy correspond with his opinion. They appear first, to have settled in their own minds what the mechanism of this organ should be in order to produce the phenomena of labour, and then set about dissecting

to confirm it; but in this they differed widely, because they had different notions of the agents required. Vesalius imagined that three strata were necessary to this end; he accordingly describes this number—one is said to be perpendicular, another transverse, a third oblique. Malpighi supposed the phenomena could be best explained, by the fibres of the uterus having a reticulated distribution; he, accordingly paints them of this form. Ruysch thought, that the fundus required the fibres to be so arranged as to form an orbicular muscle; he therefore says this part is so constructed; while the late Dr. Wm. Hunter was led to view the fibres placed in concentric circles around each fallopian tube, and in the body to have a transverse direction, &c. &c. Thus are we led to believe that a preconceived opinion has but too powerfully *biassed* the anatomist, and led him to conjecture, rather than to discovery. For it will not be pretended that either of the men whose names we have just mentioned, lacked in industry or failed in opportunities; and to have cleared up the mystery that envelopes this subject, could this have been effected by these means, would have been a triumph that either would have been proud to have won. What have they achieved? certainly not the end proposed—for all cannot be right; and much do they differ in their statements, from each other. We think it fair to conclude from this discrepancy, that they were not acquainted with the true anatomy of the uterus, at least, as far as regards the distribution of its muscular fibres. We cannot help therefore recommending the prosecution of this subject to those whose opportunities may afford a chance of success, for we feel it a desideratum to be intimately acquainted with the minute structure of this wonderful viscus; and are well persuaded much remains yet to be done before a thorough acquaintance with it will be acquired, notwithstanding Mr. Bell seems satisfied with the knowledge he has of it. He has accordingly deduced consequences from what he supposes to be the arrangement of the fibres of the uterus, but to which we cannot tacitly subscribe. We again repeat that, in our opinion, much remains to be done; nor should any one be prevented from the prosecution of his enquiries on this subject by the opinion of Mr. Burns, (Gravid Uterus, p. 44.) " that the

course of the muscular fibres is of very little consequence to the accoucheur." We are of opinion that every anatomical fact is of importance either mediately or immediately; and to the accoucheur every thing relating to the uterus is, and always must be, of use; for if it be not useful to him, to whom will it be useful? he might as well have said in so many words, "that the anatomy of the uterus is of no importance to be understood by any one."

But in insisting on the utility of investigating the structure of this part, we are happy in being supported by the authority of Mr. Bell himself; for in p. 38, he says, "The prevailing notion that the muscular fibres of the uterus are very confused and scarcely perceptible, has prevented authors from founding rules of practice on the sure ground of anatomy. And if it be possible to place this matter in a clear light, it may banish, perhaps, a certain vagueness which is much to be regretted in so important a department of practice." In this we most cordially agree; and are happy in making use of Mr. Bell's opinions where they tend to support our own. But though we have employed these sentiments to strengthen what we wished to urge, we are not of opinion that he has succeeded better than his predecessors in placing his practical deductions in "a clear light," for we again insist much remains to be done before this subject can be well understood. We will however proceed with the more immediate object of this paper, and let Mr. Bell always speak for himself. "The most curious and obviously useful part of the muscular substance of the uterus," says Mr. B. "has been overlooked; I mean the outermost layers of fibres, which cover the upper segment of the gravid uterus. The fibres arise from the round ligaments; and, regularly diverging, spread over the fundus, until they unite and form the outermost stratum of the muscular substance of the uterus." But what makes this "the most curious and obviously the most useful part" of the uterus, we cannot comprehend—for certainly there is nothing more curious in the distribution of the fibres of this part than of any other part, and from Mr. Bell's description of them we should rather be led to consider them less "curious," because, agreeably to to him, they arise from the round ligaments and regularly di-

verge over the fundus. From this it would appear that these fibres are every way obvious, since both their origin and course are well defined. We would ask then, if both their origin and course be so well marked, how it has happened that they have been so long hidden from the eyes of other anatomists? For they cannot be the orbicular muscle of Ruysch; the perpendicular one of Vesalius; the reticulated one of Malpighi; nor the concentric ones of Dr. Hunter. But let it be admitted that Mr. B. is really correct on this point; is he so, in the offices he assigns them? "The round ligaments of the womb have been considered useful in directing the ascent of the uterus during gestation; so as to throw it before the floating viscera of the abdomen: but, in truth, the uterus could not ascend differently; and on looking to the connection of this cord with the fibres of the uterus, we may be led to consider it as performing rather the office of a tendon, then that of a ligament. It is familiarly known, that the subsiding of the belly in pregnancy, occasioned by part of the womb sinking within the brim of the pelvis, is the least equivocal sign of the approach of labour, and of the pelvis being of due dimensions: and in some measure this is also an assurance of the right presentation of the child. This layer of muscular substance operating on the round ligaments, is well calculated to assist in expelling the fœtus; but also in a particular manner it is provided for bringing down the womb in the first stage of labour, and is well calculated to give the uterus and head of the child the right position with regard to the axis of the pelvis." From this it would seem that the offices hitherto assigned the round ligaments are not the true ones—that they act the part of tendons; 1st. they serve to depress the uterus in the pelvis; 2d. they assist in the expulsion of the fœtus; 3d. their action in some measure serves as a criterion of a right presentation; 4th. they bring down the womb in the first stage of labour; 5th. they serve to give the right position to the uterus and head of the child, with respect to the pelvis, &c. Let us now enquire into the truth of the multiplied offices attributed to the round ligaments and the muscular fibres originating from them. With respect to the first we deny their exclusive agency for several reasons; 1st. we believe them insufficient for this purpose;

2d. we think their direction would not be favourable thereto; 3d. because we believe we can show that the whole mass of uterine fibres contribute to this end. We believe them insufficient because, agreeably to Mr. Bell's own statement, they are but partially distributed, and only "cover the upper segment of the uterus," and consequently can have but a limited power on it—to be effectual they must have the co-operation of the other portion of this viscus; we cannot therefore admit them to be more "obviously useful" than any other portion. Their direction would very often preclude the possibility of their being useful in the manner indicated; for instead of their being "well calculated to give the uterus and the head of the child the right position with regard to the axis of the pelvis," we should find that, in almost every instance except a first pregnancy, they would throw the fundus over the pubes and place the orifice at the projection of the sacrum or in its hollow. To perform the duties assigned them by Mr. B. they should possess very strong powers of contraction—but if these were exerted with due force, the round ligaments would be put upon a severe stretch; a situation highly dangerous to their structure. For, with the exception of Mr. B. they have been universally considered highly vascular—so much so in the estimation of some, that they have been considered almost entirely composed of vessels, and that the size of their vessels augment during pregnancy and become uniformly softer. Now, can we perceive any thing in this arrangement, corresponding with the duties and structure of tendons? Should the contraction of these fibres be as powerful as their alleged duties would exact, would there not be the greatest risk of a rupture of some of their vessels? certainly there would—and can we imagine nature so idly capricious as to increase this risk, by augmenting their vascularity? That they perform their share in the expulsion of the child is admitted, as we have little or no doubt but that every portion contributes to this end. And we farther believe, that the true cause of the subsiding of the abdominal tumor is, that some time previously to the manifestation of pains which certainly indicate labour, the uterus is in a state of pretty strong, but alternate contraction; and that this is excited in consequence, as it were, of the

uterus refusing to yield any longer, after having had its fibres put to an uneasy stretch, and thereby stimulated. The first effects of these contractions are felt by the blood-vessels of the uterus; they are compressed slightly and in consequence transmit less blood—the volume of the uterus is thus diminished, and will now in part enter the superior opening of the pelvis.

That this is the reason of the "falling" as it is termed, is rendered almost certain, by the phenomena now presented. If a finger be introduced into the os tincæ and gently pressed against the membranes, it will be found that they are alternately tense and relaxed, as is also the mouth of the uterus now in contact with the finger. If the hands be placed on the abdomen the same kind of sensation is perceived—that is, we find the uterine globe occasionally harder and softer. Now, were these appearances produced by the contractions of the fibres that originate from the round ligaments, and that spread themselves upon the "upper segment of the uterus," we should find this body alternately approach and recede from the lower strait---but this is not the case; and to account for this set of fibres producing the phenomena just mentioned, we must suppose them to be in a state of permanent action, while all the other fibres are alternately contracting. The second use assigned these fibres by Mr. Bell, we generally agree to, not because they "originate from the round ligaments and spread themselves upon the upper segment of the uterus," but because they are muscular fibres and constitute a portion of the uterus, every part of which, it is our creed, contributes towards the expulsion of its contents.

Moreover we cannot agree in the declaration of Mr. B. when he says that the sinking of the uterus within the pelvis "is the least equivocal sign of the approach of labour," for assuredly there are signs, which direct the accoucheur with much more certainty, and were this the proper place several could easily be pointed out.

The third advantage derived from the action of this set of fibres and tendons is, that when exerted so as to depress the uterus "within the brim of the pelvis," we have "in some measure an assurance of the right presentation of the child." We scarcely know how this idea could be suggested for a mo-

ment, let alone gravely and deliberately advanced. To make the action of these fibres an evidence of a "right presentation," is to declare a connection between "the right presentation" and their action. What evidence has Mr. B. of this? We unhesitatingly say, none; for we assert without fear of contradiction, that the uterus falls "within the brim of the pelvis" with as much certainty when another part than the head presents, as when this latter portion of the child is to the os uteri. For there are but two reasons why this circumstance does not always obtain; first, where the uterine fibres are indisposed or incapacitated, to take on that action which in our view is essential to produce "falling;" secondly, where the uterus is too large either positively or relatively, to enter the pelvis; now, it is well known that neither of these causes have any necessary connection with the species of presentation.

The fourth office assigned by Mr. B. is that these fibres bring down the womb in the first stage of labour. Here we find a little difficulty to arrive at Mr. B.'s true meaning. If Mr. B. means by the first stage of labour what we presume he does, "the sinking of the womb within the brim of the pelvis," for he declares this "to be the least equivocal sign of labour," we shall merely refer to what we have said when that subject was under examination; but if he means the more advanced stage that most accoucheurs denominate the first stage,* we should say that the use he assigns to these fibres would be injurious, as the lower part of the uterus would constantly be driven down before the presenting part, to the manifest delay of the labour.

The fifth use attributed to these fibres by Mr. B. is that they serve to give the right position to the head and uterus as regards the pelvis. Was the right position of the child and uterus to depend solely upon any influence these fibres could possibly exert, we very much fear that the act of child-bearing would not only be much more tedious, but also much more dangerous. For, if we are correct in our estimate of their powers, and mode of operating, we should find them, as has

* Namely the opening of the mouth of the womb; secretion of mucus; pain, and gathering, as it is termed, of the waters.

already been observed, drawing the uterus out of the line it should take to give facility to labour. And were we to cede to them the power of regulating the direction of the uterus, we could not possibly grant them the power of doing this for the head; since it is well known to every practitioner of midwifery that while the membranes remain entire, the head may alter its position twenty times a day.

Mr B. says farther that, if it were not for the mechanism he describes, "we should be at a loss to conceive how the uterus, by its own action, could adjust the position of the orifice for the delivery of the child." We would ask is there any power that does this with exactitude? we should say we know no such power. Mr. B. has confessed himself no accoucheur; were he one he would have known that the uterus is subject to a displacement called "obliquity;" and that displacement obtains in a greater or less degree in almost every pregnancy. The body and fundus may incline to the right, or to the left, or anteriorly, while the orifice will be found in the opposite direction; so that the axis of the fundus and mouth rarely correspond with the axis of the pelvis, nor does this departure create any serious inconvenience, as it is easily remedied when necessary, and this without the aid of the round ligaments, or the fibres emanating from them. In a word we think Mr. B. has mistaken the offices of the round ligaments; and should the duties hitherto assigned them be not the true ones, we think those attributed to them by him, are quite as questionable.

In p. 39, Mr. B. observes that "after making sections of the substance of the womb, in different directions, I have no hesitation in saying, that towards the fundus the circular fibres prevail; that towards the orifice the longitudinal fibres are most apparent; and that on the whole, the most general course of the fibres is from the fundus towards the orifice. This prevalence of longitudinal fibres is undoubtedly a provision for diminishing the length of the uterus; or for drawing the fundus towards the orifice. At the same time these longitudinal fibres must dilate the orifice, and draw the lower part of the womb over the head of the child."

To this statement of the arrangement of the muscular fibres

of the uterus we can agree, but cannot consent *entirely* to Mr. Bell's explanation of their uses. We do not believe that the longitudinal fibres serve to "draw the lower part of the womb over the head of the child," because we see no such effect during the time of pain; on the contrary we invariably find that that portion of the uterus which surrounds the child's head, to be pushed with it lower into the pelvis during each pain, and the child retained nearly in the place to which it was driven by the alternate contractions, is owing to the tonic contraction of the uterus; and this last takes place in proportion as the uterus becomes emptied, for it is an inherent principle of this organ when in healthy condition, to lessen itself, as the distracting cause is removed; and to this latent power, the name of tonic contraction has been given, to distinguish it from the one which is accompanied with pain, and called spasmodic. It is true that we sometimes see a portion of the uterus suddenly retract itself when set at liberty, after having been detained and stretched by the presenting part, but this is owing to its have been unusually elongated and compressed between the head and pelvis; when then you relieve it from this state of durance, it will sometimes instantly escape, but not always; to the sometimes great inconvenience of the accoucheur. And we think we may here challenge a solitary instance in the practice of any man, of the mouth of the uterus being literally drawn over the head of the child. That it eventually passes over it we admit, because the head must pass through it before delivery can be effected. In strict parlance then, it is the head passes through the mouth of the uterus, and not the mouth being drawn over the head. Indeed, were this strictly to take place as Mr. B. says it does, it would be attended sometimes with great inconvenience; as this part would then be left at liberty to embrace the neck of the child, and thus offer great obstruction to the passage of the shoulders.

At p. 40, Mr. B. observes that a very principal effect of the muscular action of the womb is the constringing of the numerous vessels which supply the placenta, and which must be ruptured when the placenta is separated from the womb. To us it is really a matter of much surprise, situated as Mr. B. is with

every source of information at hand, that he should talk of the rupturing of vessels on the separation of the placenta from the uterus. We should have imagined that Mr. Bell, from his own observations, would have been able to correct the errors of the older writers on this subject; or had these failed, he surely might have been set right by almost any late author on the subject of midwifery. We convey no information at this day, when we say that there is no rupturing of blood-vessels by the separation of the placenta from the uterus; for it has been satisfactorily proved there is no inosculation of their respective vessels; that there is a greater or less discharge of blood after this process we admit, but this is from the mouths of vessels which remain patulous until closed by the tonic contraction of the uterus. We scarcely need ask what would be the consequences did this rupturing really take place. Inflammation, suppuration and gangrene would be the melancholy suite in most instances of otherwise healthy labours. But if this opinion raise our wonder, how much more powerfully should it be excited at the following limited and novel view of the uterus!

Mr. B. p. 40, says, "I have observed further, that although in producing contraction and thickening of a portion of the uterus, by boiling it, or by other artificial means, the fibres are made very evident, and the blood-vessels greatly constringed; yet they are not so effectually closed as in the natural contraction of the muscular fibres of the uterus. Thus," continues he, "we are led to contemplate the uterus as more peculiarly destined for the safe delivery of the secundines, than for the reception and growth of the ovum." Does the conclusion legitimately flow from the premises? certainly not—Because the natural contraction of the uterus will constringe its blood-vessels more effectually, than boiling will, it follows agreeably to Mr. B. that the uterus is "more peculiarly destined for the safe delivery of the secundines, than for the reception and growth of the ovum!" The fitness of the uterine fibres for distention; the gradual, beautiful, and regular order of their development; the protection afforded by them as a covering; the wonderful increase of capacity in the exact proportion to the necessity; the mysterious and hidden mode by which nourishment is prepared and conveyed; the production of the decidua;

the engrafting of the placenta; the almost perfect safety from external accident afforded by the waters of the amnion, all fail to make Mr. B. see that the uterus is as well calculated for the reception and growth of the ovum, as for the safe delivery of the secundines; and why? forsooth, because a piece of boiled uterus would not shut up its vessels as closely as if it were living!

"I have not succeeded," says Mr. B. p. 42. "in discovering circular fibres in the os tincæ, corresponding in place and office with the sphincter of other hollow viscera, and I am therefore inclined to believe that, in the relaxation and opening of the orifice of the uterus, the change does not result from a relaxation of muscular fibres surrounding the orifice. Indeed it is not reasonable to conceive that the contents of the uterus are to be retained during the nine months of gestation by the action of a sphincter muscle. The loosening of the orifice, and that softening and relaxation which precedes labour, is quite unlike the yielding of a muscular ring." This declaration of Mr. B. amounts but to this negative, that he did not find muscular fibres in the os tincæ--but does it follow they do not exist? Do not all the phenomena exhibited by this part during pregnancy and parturition satisfactorily prove they do exist? To what shall we attribute the resistance of this part in many cases of labour and abortions, but to the contraction of muscular fibres? To what shall we give the power, closing to a certain extent, the mouth of the uterus during each pain, but to muscular fibres? Or what is still more in point; what closes the mouth of the uterus upon the finger in the beginning of labour, in the absence of pain, when we gently irritate it? The answer we believe to be evident. Should the "loosening of the orifice, and that softening and relaxation which precede labour," be "quite unlike the yielding of a muscular ring," the contraction which almost invariably takes place with the accession of pain, certainly is not. It is completely familiar to every accoucheur, that during "a pain" the edge of the mouth of the womb becomes stiff and offers very considerable resistance for a long time. On what does this stiffening depend? we say upon the contraction of the muscular fibres which surround the os uteri.

Does it follow because Mr. B. thinks it "unreasonable to conceive that the contents of the uterus" can be "retained during the nine months of gestation by the action of a sphincter muscle," that that muscle does not exist? Is not the uterus, from the very important duties which it performs, entitled to its own particular powers and arrangements as well as another part of the human system? As well might Mr. B. declare the heart to be no muscle, because every other muscle has its interval of rest, because subject to fatigue, while the heart unweariedly continues its duties. Analogy may be safely and sometimes advantageously employed to strengthen an explanation, but the want of it should never be considered sufficient to destroy a fact. But would running a parallel between the powers of the os tincæ and other "muscular rings" weaken our reasons for supposing this part to possess them? Certainly it would not. Thus the sphincter of the anus and that of the bladder, are intended to contract and relax at short intervals; and when in a healthy state, do completely perform their duty; while the sphincter of the uterus, for we may so term it, is designed to maintain its contraction until that state shall be no longer necessary, and this in the healthy state is for the period of nine months. Should it relax sooner than this, more or less injury is done: but is this not precisely the case with the other sphincters? Does not mischief arise from the too frequent or the too rare relaxation of these parts?

We shall now notice another error of Mr. Bell's, into which he could not have fallen, had he had a knowledge of midwifery. In p. 42, Mr. B. says, the first movement of the uterus "is to shift its position to direct the orifice aright, and to sink down until the lower segment of the womb rests upon the brim of the pelvis: this is done by the operation of the muscular fibres on the round ligaments, and during this shifting of its position are experienced the true dolores præsagientes." Now any practitioner could have told Mr. B. that this movement frequently takes place two or three weeks sometimes before the slightest "pain" manifests itself; and this change is effected when every thing is well disposed, without pain, for the most part, being either an essential or a concomitant. And when this change does not happen, we are not to conclude that there is necessarily either a mal-presen-

tation or a deformed pelvis, but simply to consider that some interruption has been given to the general contraction of the uterus.

As this paper has extended much beyond the length we first contemplated it would, we shall pass without notice several observations of Mr. Bell's which pretty loudly call for animadversion, that we may spend a few minutes on his "improvement" in performing the cæsarean operation. In p. 48. Mr. B. gives the history of the dissection of a case in which this operation had been unsuccessfully performed by his brother Mr. John Bell. In p. 49, Mr. B. observes that "the circumstances of this case suggest very material improvements in the manner of performing the operation." He advises "instead of cutting into the womb, as was done in the preceding case," that a very small incision should be made, such only as would enable a finger to be introduced; by boring, aided by the disposition which the uterus has to dilate, another finger might be passed, and then a third, and at length the whole hand, in a conical form, might be forced into the womb. I further venture to suggest," continues Mr. B. "that this should be done as low down upon the lower part of the uterus, as the urinary bladder and the reflection of the peritoneum will permit; for at the lower part the uterus is least vascular, and most disposed to dilate. In the further prosecution of the operation, the child should not be suddenly extracted, but the feet being brought out by the opening, the body should be slowly delivered by the spontaneous action of the womb; and the whole operation performed as much as possible in imitation of the gradual progress of a natural labour: the only apology for hurry in the operation would be the separation of the placenta, or the compression of the cord in the narrow wound; but the placenta could not possibly be detached in the method now proposed, and it would be possible to guard against the compression of the cord."

We have chosen to give the words of Mr. B. at length, that no misunderstanding should arise from our relation of his "improvement." What is the "improvement" proposed? That a "small incision should be made" instead of the extensive one that has hitherto been used. Relying "on the disposition

which the uterus has to dilate*" for the amplification necessary to deliver the child, &c. through it, Mr. B. directs the incision to be no larger than "would enable a finger to be introduced," but that this is to be gradually augmented "by boring" so as to admit the hand in "a conical form." How, and at what expense is this to be obtained? Certainly not by any "disposition the uterus has to dilate" at this time for it is now at its maximum of distention. Can it be supposed that under such circumstances, a hole capable of barely admitting a finger, can be increased so as to admit the whole hand, but at the certain hazard of tearing or rupturing the substance of the uterus? If this be true (and we think it cannot be doubted) what advantage has Mr. Bell's method over the old one? We say none —on the contrary we think it highly injudicious and dangerous. It has ever been a point yielded, that much more mischief results from distracting or lacerating a part, than from cutting it by a sharp instrument; a part treated as directed in this new method of operating, would then be precisely in the condition of a lacerated one, and of course would have to suffer all the "penalties" arising from this outrage.

In cases where (reasoning *à priori*) we should apprehend much less injury would be done to the substance of the uterus, we are cautioned with no common earnestness by the most experienced accoucheurs not to run the risk; we mean in certain cases of "floodings" where the mouth of the uterus is not sufficiently dilated to allow the hand to pass freely. We are directed with much propriety in such cases, not to employ too much force in dilating the os tincæ, lest inflammation with its consequences should ensue. And our own observation has furnished us with more instances than one, where the most melancholy effects have followed this ill directed manœuvre, If then, mischief may arise from the attempt to dilate with too much force a part from its structure capable of being dilated,

* It is certainly new to us that the part indicated by Mr. B. for the incision (the lower part of the uterus) is more disposed to dilate than the upper part of this organ, and are at loss to discover on what he grounds this *assertion*. As far as the order of the development of the uterus from pregnancy will aid us, we think we have a right to declare that the lower portion of the uterus is less disposed to yield than the upper, as we uniformly find it in health to be the last that unfolds; and that this is an established provision of nature for the security of the ovum. Where this does not obtain, abortion is the uniform consequence.

how much more is to be apprehended from an attempt upon a part not fitted by its organization for this purpose? In the one instance we are merely stretching the fibres of a part in the direction they are known under proper circumstances spontaneously to yield; while in the other, we are obliging them, at the expense of their continuity, to give way to the distracting power of a wedge. That every evil consequent upon a severe laceration would follow this practice, we have every reason to fear; what then is gained by this "improvement?"

Hitherto we have been considering this practice under its most favourable aspect, that is, where the placenta offers no difficulty to it from its location. What then must be the objections where this mass is attached to the anterior part of the uterus? The difficulties we have just stated will not only obtain, but also those that must necessarily arise from forcibly detaching this body from its attachment with the womb. For when thus situated, it must either be detached, or penetrated; if detached, hæmorrhagy to a greater or less extent must ensue; if penetrated, (which by the by would be a work of much difficulty) much embarrassment would be experienced in extracting the child.

But let us admit that the hand has passed into the uterus and the feet seized, agreeably to the direction of Mr. B., how is the child to be extracted through the opening made for it? The child may be *forced* to pass as far as the axillæ; but how are the arms to be delivered? Will it be maintained that an aperture no more than sufficient for the hand to pass through in "a conical form," will be capacious enough, without further violence and mischief, to permit the exit of a child's body at the full period of gestation? How will the hand, or even two or three fingers be passed over the shoulders with a view to bring out the arms, since we must suppose that the artificial opening is now *completely* occupied by the body of the child? How are we to gain additional room to bring them from within the orifice of the wound? But let us suppose all these difficulties surmounted, how is the head to be relieved, or how obviate the compression on the umbilical cord? The "disposition which the uterus has to dilate," and on which Mr. B. seems almost exclusively to rely for the success of his operation, is not more

remarkable, cæteris paribus, than its tendency to contract when freed from any distracting force. If this be true, and we think it will not be questioned, no sooner would the shoulders be liberated, than the poor infant's neck would be instantly encircled by the artificial ring intended for its emancipation, and be, to all intents and purposes, entrapped in the pillory, which had been so dexterously made by the surgeon, of its mother's womb. In vain would we obey Mr. Bell's direction to "imitate as much as possible the gradual progress of a natural labour when attempting to extract the child, for the efforts of the uterus would lend us no aid, as they would be directed in a contrary course agreeably to the opinion of Mr. B. himself; for he has confessed the action of the uterus to be from fundus to mouth. Are we to expect a miracle wrought, because this improvement has been resorted to? Are we to expect, that the order of this organ's functions will be changed expressly to favour this new operation? In a word, how is the head to be relieved? We believe it cannot be delivered; unless indeed so much injury has been done to the uterus as to completely paralyse its natural powers. And in this case, we scarcely need say, it would be worse than Utopian, to expect any benefit to the mother from this operation. Where then is the advantage of this "improved" method of operating? We see none, nay, worse than none; for in the old method, both a mother and a child have been sometimes saved; but in this, we have no hesitation to declare it our belief, that mother and child would inevitably perish. The risk on the part of the child is most unquestionably increased, for it must suffer all the dangers that would arise from being delivered "footling" from a small pelvis or rigid soft parts, without even the equivocal benefit of the use of the forceps. It would seem that every step we take in this new operation offers but fresh difficulties; for let us suppose the child delivered; the placenta is yet to follow. If the uterus be in a state of atony when we separate this body—for separate it we must; for Mr. B. says "that the placenta could not possibly be detached" (spontaneously) "in his method"---the woman would sink from hæmorrhagy;---if it retain its powers of contraction, the artificial opening will be reduced to nearly the original size made by the knife. How in this case is the placenta to be delivered? Shall we be directed, again to dilate

this aperture with the hand and then seize with it the placenta? Will no difficulty be experienced in retracting the hand when loaded with the placenta?

How are we to obviate the most common, and most obvious risk, the compression of the funis, during this tedious and difficult operation? Mr. B. has told us "it would be possible to guard against the compression of the cord," but unfortunately has not detailed the method whereby this desideratum is to be obtained. He appears to have been aware of this difficulty, but has left no suggestion to obviate it; even the old method is not exempt from this disaster, as is proved by Mr. Bell's quotation of Mr. Hunter's case, but where he, Mr. B. roundly asserts, "in truth there is no disposition in the wound to contract," although Mr. H. was obliged to introduce a couple of fingers into the womb to protect the funis; for "the womb contracted round the neck of the child, so as to retard the delivery of the head and press the funis." (Note p. 49.) Here we find are two statements in the same paragraph in direct opposition to each other. Mr. Hunter, in relating his case, is supposed to state a fact; that "the womb contracted closely round the child's neck;" and he is quoted for the express purpose (as we suppose) to prove, that this does take place in this operation, and to bear Mr. B. himself out when he asserts that "the only apology for hurry in the operation would be the separation of the placenta, or the *compression of the cord in the narrow wound;*" yet he immediately after declares that, "there is no disposition in the wound to contract." How are we to reconcile this discrepancy in Mr. Bell, but by saying he had a theory to support?

But let us suppose the operation finished; what will be the condition of the poor, suffering, unresisting mother. Subjected by this method for perhaps hours* to more than inquisitorial tortures, she becomes exhausted; scarcely daring to hope a life so jeopardized by severity can be saved; or perhaps is prevented even from wishing it, by the death of her infant, whose smiles were to be the humble and only reward for all her sufferings.

Philadelphia, February 5th, 1818.

* For agreeably to the instructions of Mr. B. the body should be slowly delivered by the spontaneous action of the womb; and the whole operation performed as much as possible in imitation of the *gradual progress of a natural labour.*"

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

VACCINATION.

We offer no apology in giving circulation to the following observations of Dr. Muter, of Battle, Sussex, on the Cow Pox Inoculation, which have appeared in some of the principal publications.—The subject is of great importance; and we trust the Doctor's remarks will dissipate whatever prejudices may have been ignorantly created.

"THAT a person having had the cow pox properly, is as permanently safe from small pox as though he had undergone that disease in the natural way, however severely, cannot admit of a doubt. It is established beyond all controversy; but I confess, it is too true, that small pox often occurs to such as have been vaccinated. To render vaccination, in every instance, absolutely and permanently successful, it is necessary, 1st. That the matter used be good, that is, taken at a proper period from a pustule which has gone through the regular stages. 2d. That the specific action be excited and completed, locally, and constitutionally, without interruption; then not a case in ten thousand will fail. But to be certain of all this requires great care, and not less skill. Every medical man, and no other, ought to vaccinate—should frequently send for matter to one or other of the different institutions established on purpose to promote the interest of vaccination—for this plain reason, because he cannot depend upon what he borrows from another. He should likewise make himself master of the subject, and be able to say decidedly, this is a good or a bad pustule: here the patient is safe from small pox, or still liable to it. This can only be done at the end of two weeks, because that time is necessary to complete the formation, and absorption into the system of cow pox matter. But during this period the patient should be seen at least twice, in order to witness its proper stages. The causes of failure are concerns of the greatest importance, and should be placed in the most luminous point of view possible. Not only is the character of a medical man, but the life of the patient, at

stake; if he fails, the patient suffers from a loathsome and fatal disease, while the mistake ultimately recoils on himself. In many instances the way of a medical man with his patients is as the way of a ship on the sea, as the way of a serpent on a rock, trackless as the eagle's flight in the air, evanescent as the lightning's flash in the sky.—The patient dies, and dead men tell no tales.—Then who can judge where there is no evidence left upon which to determine; or, who can say, has the medical man done or not done his duty? In this instance, however, all are competent to decide. Has the small pox occurred to a single patient he formerly vaccinated? if so, he has not done his duty, provided that he had an opportunity to see the case repeatedly afterwards. My medical brethren, think on the responsibility to which you and I are subject; and remember, that vaccination proves one of the most delicate tests of medical acumen and assiduity.

"I shall now point out a few of the more evident causes of the occasional failure of cow pox. 1st. There is often much difficulty in deciding positively, whether the patient has been perfectly and successfully vaccinated.—2d. Medical men are not sufficiently paid for vaccination, and it is attended with great loss of time always to see the patient sufficiently often.—3d. They borrow cow pox matter from one another. This is always a cause of uncertainty, and often productive of infinite mischief.—4th. Parents are often careless, and neglect to have their children's cases properly inspected.—5th. The specific action is frequently disturbed by accident, the child rubbing off the head of the pustule before it is fully matured.—6th, Cow pox matter is sometimes taken from an arm a considerable time after the appearance of the areola.—7th. There is also a risk in taking it too soon.

"Mr. Hudson has with great propriety suggested a second vaccination in all doubtful cases. He has likewise pointed out unanswerable objections to a second inoculation with small pox.—So far we agree; but I must declare, that I consider all fear groundless as to the deterioration of cow pox matter from frequent repetition. To conclude; I have vaccinated a great number every successive year, and, to the best of my knowledge, no patient vaccinated by me has hitherto caught the small pox. Indeed, this is no more than what may be accom-

plished by every medical man, and every one who reflects on the accumulated evidence collected from every quarter of the globe, must admit that it is an indelible stain on the medical character to hear us publishing our own shame in reporting so many failures."—*Leeds Mercury, February* 7, 1818.

It appears by these very interesting observations, that the vaccine crust or scab, so generally used in the United States, is not commonly employed in Great Britain.—*Editors.*

Orfila *and* Majendie's *Experiments on Morphium.*

[From the London Medical Repository, for April 1818.]

THE fact that the *narcotic principle of opium* is morphium, an alkaline substance, *sui generis*, discovered by M. Sertuerner, is now pretty generally admitted; and although physicians in this country have not endeavoured to establish by experiments the effects of this substance on the animal economy, yet, many trials of it, both in its simple state, and in combination with various acids, have been made by various physiologists on the continent. The most celebrated of these experimenters are M. Orfila and M. Majendie, both of whom have published details to their experiments*, and it is now our object to lay an abstract of these memoirs before our readers.

In the first *M. Orfila* purposes,—"1st, to establish that the aqueous extract of opium owes its medicinal properties to an alkali composed of oxygen, hydrogen, carbon, and azote; 2dly, to compare the effects of the aqueous extract of opium on the animal system, with those of morphium in its simple state, and in its state of combination with acids, with oils, and with alcohol; and, 3dly, to point out the method of treating symptoms indicative of poisoning by morphium."

Before entering upon the physiological part of his Essay, M. Orfila describes the physical and chemical properties of morphium, which we need not transcribe, and then details the method by which Robiquet orders it to be prepared, as the best process for procuring it. "Boil, for a quarter of an hour, a concentrated infusion of opium with a small quantity of mag-

* Vide Nouveau Journ. de Médicine, et Chirurgie.—tom. i. p. 1.

nesia; a greyish precipitate will be produced, which appears to be a compound of morphium and a sub-meconate, with some colouring matter: wash it on a filter, and boil it with concentrated alcohol, which dissolves the morphium, and allows it to precipitate almost entirely as it cools. It is necessary to redissolve the morphium in concentrated alcohol, in order to obtain it in a state of purity."

The first set of M. Orfila's experiments were intended to ascertain "the action of Morphium itself on the animal economy." In two experiments, that substance was introduced into the stomach of two dogs in the doses of two and five grains; and in a third experiment it was applied to the exposed cellular tissu on the thigh of a dog: vomiting, partial paralysis, and other symptoms of poisoning by opium were produced; but the animals rapidly recovered. From these experiments, M. Orfila, finding that morphium alone, owing to its little solubility in water, exercises but a feeble action on the animal economy, next tried it in combination with some acids.

The acid with which M. Orfila first combined it, was the acetic, diluted with water. Six grains of the acetate thus formed were introduced in the stomach of two dogs. In a short time the posterior extremities were paralysed; the animals seemed to be asleep, but were roused by the smallest noise; and, as if terrified, made fruitless efforts to escape; but as they fell in the attempt, they appeared instantly again asleep. The pupils were dilated, the pulse was slow, and the respiration labouring. At the end of eight hours they uttered a piercing cry; but on the following day all the symptoms diminished, and the animals gradually recovered their functions.

"Another dog was treated in the same manner, with six grains of the aqueous extract of opium dissolved in half an ounce of weak acetic acid. The posterior extremities became so enfeebled that the animal walked with difficulty; there was a slight tendency to sleep, but the respiration remained unaffected, and next day the animal appeared in perfect health.

Six grains of morphium dissolved in vinegar being injected into the cellular tissu in the thigh of a small dog, nearly the same symptoms were produced as when the acetate was introduced into the stomach; but the feebleness of the hind legs

did not altogether subside until the second day afterwards: and when twelve grains of the acetate were employed, the animal did not recover its ordinary state of health until the close of the fifth day. When six grains of the aqueous extract of opium dissolved in the acetic acid was injected into the thigh, in the same manner as the acetate of morphium, the symptoms subsided the next day; and on the third day, when the dose amounted to twelve grains.

Two grains of morphium, dissolved in water slightly acidulated with vinegar, were injected into the jugular vein of a stout dog. "The only symptom produced was paralysis of the posterior extremities; which terminated on the evening of the same day. On substituting two grains of the aqueous extract of opium for the morphium, no effect whatever was produced.

"One grain of morphium was dissolved in double the quantity of acidulated water, as in the former experiment; the effects produced were, at first, paralysis of the posterior extremities, and then drowsiness. These symptoms were augmented in a quarter of an hour; but they subsided in five hours, and on the following day the health of the animal was completely re-established. A dog of nearly the same size and strength was instantly killed, by the injection of two grains of morphium dissolved in acidulated water. Two grains of recently prepared aqueous extract, dissolved in the same quantity of acidulated water, and injected into the jugular vein of a robust dog, produced paralysis of the posterior extremities, drowsiness, and impeded respiration. These symptoms, however, subsided in an hour, and the animal walked with ease."

From these experiments M. Orfila has drawn the following conclusions: "1°, that morphium dissolved in acetic acid produces the same symptoms as the aqueous extract of opium; which inclines him to believe that the active ingredient of the extract is a salt analogous to the acetate: 2°, that the acetate of morphium exerts a more intense action on the animal economy than the same dose of the aqueous extract of opium. But it is necessary to reflect, that if twelve grains of the aqueous extract of opium does not occasion so violent an effect as twelve grains of morphium dissolved in vinegar, it is owing to that quantity of the extract containing, besides the morphium,

two acids, some extractive, &c., which necessarily reduce the proportion of morphium to considerably less than twelve grains. It is, therefore, extremely probable that if twelve grains of this alkali were dissolved in the peculiar acids contained in the aqueous extract of opium, more intense effects would be obtained than result from twelve grains of morphium dissolved in acetic acid. This would also necessarily occur, if the acid which is combined with the morphium in the aqueous extract of opium did not neutralize it as well as the acetic acid; for it is evident that, in this case, the morphium being more free, would exert a greater intensity of action.

"With a view of trying the effect of another acid, twelve grains of morphium dissolved in water, slightly acidulated with sulphuric acid, were applied to the cellular substance on the thigh of a dog. In six minutes the posterior extremities were paralyzed and the pupils dilated; a profound sleep appeared to follow, from which, however, the animal was roused by the least noise. These symptoms declined in six hours, and on the following day the animal walked without any difficulty. On substituting *hydrochloric acid* for the sulphuric, some vertigo and feebleness of the hind legs supervened at the end of two hours, and in another hour the prostration of strength was complete, and succeeded by the same symptoms as in the preceding experiment. On the following day the animal appeared in perfect health.

"These experiments prove that the sulphuric and hydrochloric acids more completely neutralize the poisonous properties of morphium than the acetic acid, since the effect produced by an equal dose of the acetate is more intense than that which follows the employment of the sulphate and the hydrochlorate. They also demonstrate the error of *M. Ridolphi*, in asserting that acids are the best antidotes of morphium; and that animals might swallow, with impunity, combinations of morphium and the acetic, nitric, and hydrochloric acids*."

The next set of experiments made by M. Orfila, had for their object to determine the effect of *morphium dissolved in oil of olives*. Very violent effects were produced when six grains only were swallowed; and by augmenting the dose to

* Vide Brugnatelli's Journal, December 1817.

twelve grains, death followed in eighteen hours after the mixture was swallowed. The same result also followed the injecting of a solution of twelve grains of morphium in an ounce of olive oil into the cellular substance on the thigh of a dog; and likewise by the injection of a grain of it dissolved in a drachm of oil. These results appear to prove that oil neutralizes the poisonous property of morphium in a much less degree than acetic acid, or even than the sulphuric and hydrochloric acids.

"The quantity of morphium which *concentrated alcohol* can dissolve is so small, at the ordinary temperature of the atmosphere, that when diluted with a moderate quantity of water, the mixture must contain, comparatively speaking, scarcely any morphium. In experimenting, therefore, with this solution, it is by no means certain whether the effects should be attributed to the alcohol or to the morphium; but there is no doubt that the quantity of alcohol necessary to dissolve three grains of morphium is sufficient of itself to kill any dog to whom it might be administered. The case is different with man, who is accustomed to the use of spirituous liquors: and, from the experiments of M. Sertuerner himself, who took, in three quarters of an hour, a grain and a half of morphium dissolved in a drachm of alcohol diluted with several ounces of distilled water, it acts powerfully in this combination.

"On depriving the aqueous extract of opium of its morphium, eighteen grains of it, introduced into the stomach, and applied to the cellular tissu on the thigh of a dog, produced scarcely any sensible effect."

From the whole series of experiments, of which we have attempted to give an abstract, M. Orfila concludes—

"1, That morphium may be introduced into the stomach of even weak dogs, to the extent of twelve grains for a dose, without producing any very sensible effect; whilst the same quantity of the aqueous extract of opium produces violent symptoms of poisoning and sometimes even death. This inertness of morphium depends on its little solubility, and the consequent difficulty with which it is attacked by the gastric juices.

"2, That the salts of morphium which are soluble in water, such as the acetate, the sulphate, and the hydrochlorate, act

very much in the same manner as the aqueous extract of opium; which induces the belief that the effects of that medicine ought to be attributed to the salt of morphium, and that this is probably the meconate discovered by M. Sertuerner, and the existence of which has been confirmed by M. Robiquet. This important fact ought to lead us to search for morphium in indigenous plants, and to separate it for the purpose of transforming it into salt, and for substituting that for the watery extract.

"3, Morphium dissolved in the acetic acid exerts upon the animal economy a more intense action than the same dose of the aqueous extract of opium; a phenomenon which proves that the extract contains other principles besides morphium.

"4, The aqueous extract of opium, from which the morphium has been separated, may be administered in a large dose without producing symptoms of poisoning. And if it sometimes exhibits a slight action, it is owing to the separation of morphium being incomplete.

"5, Six grains of morphium dissolved in oil of olives, appear to act with more intensity than twelve grains of the aqueous extract of opium, which proves that oil neutralizes much less the poisonous properties of morphium than the acids. This fact is remarkable, and affords the means of doubling, in some degree, the medicinal properties of the aqueous extract of opium; a result which is quite novel.

"6, Morphium, like all other substances which produce their effect after being absorbed, exerts a more intense action when it has been injected into the veins, than when simply applied to the cellular tissue, or introduced into the stomach.

"7, The poisoning produced by morphium shows itself by the same symptoms as that occasioned by opium, and requires to be treated in the same manner. The poison must first be expelled by emetics, and afterwards vegetable acids considerably diluted, with coffee, &c. freely administered. These means, sometimes, aided by bleeding at the jugular vein or the arm, will restore the patient.

"8, Alcohol diluted to the degree which is necessary to prevent it from acting as a poison to dogs, dissolves so small a quantity of morphium, that it is impossible to ascertain the ef-

fects of such combination upon these animals. But it is nevertheless probable that the alcoholic solution of morphium, may be successfully employed by man, who being in the habit of taking spiritous liquors, can take a large dose of diluted alcohol without any inconvenience."

"The paper of M. Majendie, on the subject of morphium, is in some degree more interesting than that of M. Orfila, in as much as it exhibits the effects of this important agent on the human economy in the state of disease. After some preliminary observations, our author details the following case.

"A lady, twenty-four years of age, had been afflicted for ten years with a complaint which I consider to be an aneurism of the thoracic aorta. She had been treated at different times by well informed physicians, by quacks, by pharmacopolists, by magnetizers, herborists, &c.; and she had, strictly speaking, tried all the resources of art and of empiricism. She was, nevertheless, tormented with constant wakefulness, and extremely acute pains in the region of the diaphragm, as well as in the lower extremities, which were very much wasted.

"I at first employed prussic acid with some advantage, but its use could not be continued more than six weeks, as it occasioned delirium, pains, and anxiety. I then decided on trying the salts of morphium, the narcotic powers of which I had ascertained from experiments on the lower animals: and having procured some which had been prepared by M. Planche, I prescribed four pills, containing each a quarter of a grain of acetate of morphium; and requested the patient to take one every night and morning.

"The first night, she took a pill on going to bed; but finding no sensible effect from it at the end of half an hour, she took a second. Some minutes after she had swallowed it she fell into a profound sleep, which she had not experienced for many months. She slept quietly for three or four hours, but awoke towards the middle of the night, and complained of severe nausea. She however, soon slept again. This occurred several times successively; and at the end of six hours, violent efforts of vomiting coming on, she rejected a small quantity of mucus and bile. She did not again sleep, but remained in a

quiescent state which she had not before experienced; and during the whole of the night she had been altogether free from pain.

"In the morning, when I saw her, both herself and her parents expressed the greatest satisfaction for the quiet night she had passed; and were willing to repeat the medicine. It was, however, evident that the medicine had been overdosed; and therefore, resolving to moderate the dose, I consequently ordered four pills, each containing an eighth of a grain of acetate of morphium, and directed the whole to be taken in twenty-four hours. In this manner I obtained the sedative effects of the medicine without its deleterious results.

"The patient took these pills for six months, and always with advantage. She regulated the number by the effects produced; and, what may appear remarkable, the continuance of them did not weaken their effects: nor can she even now take more than four in the twenty four hours, without experiencing some inconvenience, such as violent cephalalgia and nausea.

"I tried, with the same patient, to employ the muriate of morphium instead of the acetate; but the experiment did not succeed; for it was necessary to take a grain and a half of that salt to produce its narcotic effect, which was, even then, imperfect: and the patient being sensible of this difference, was not desirous of continuing its use.

"The sulphate of morphium, which I have also tried on the same person, has a more feeble action than the acetate, but much greater than that of the muriate: its narcotic power is also more complete, the sleep which it procures being more free from delirium: in a word, its mode of acting approaches more to that of the acetate, although it is sensibly less energetic.

"The patient continued to use it for four months in conjunction with the acetate. She named the acetate "*her stronger pills*," and the sulphate "*her weaker pills;*" although each contained the same quantity of the salt, viz. the eighth part of a grain. According to the degree of her sufferings, or the quantity of sleep which she enjoyed, she took the stronger or the weaker pills, and sometimes combined the action of both kinds.

"About three weeks ago the patient, as is often the case with those who are long afflicted, expressed a desire to change the remedy, and requested me to give her some other pills. I proposed to her the gummy extract of opium, the effects of which I had compared with the salts of morphium: but she objected to the formula, assuring me that she had frequently taken it; and that the preparations of opium were always hurtful to her. Supposing that her imagination had deceived her in this respect, I proposed the essential salt of Derosne, without informing her what it was: she consented to take it; but I was soon convinced that what she had stated regarding the effect of opium was correct; for half a grain of the essential salt, which she took in four pills in the course of twenty-four hours, excited extreme agitation and the most intense cephalalgia. The patient, therefore, returned to the use of the salt of morphium, and has continued it to this day.

" I have employed the above mentioned salts of morphium with the most decided benefit. I may mention, among other cases, that I gave the acetate to a lady who had a scirrhus of the left mamma, and refused to submit to the operation. For two months she took a quarter of a grain of the acetate of morphium every day, without any other remedy: the lancinating pains which had tormented her were greatly subdued, and the intervals between them much lengthened.

" From these facts, I am of opinion that the acetate and the sulphate of morphium may be advantageously employed as narcotic medicines."

Mr. SAMUEL YOUNG has in the press, further Reports of cases of Cancer successfully treated by his new method of Pressure.

JOHN SYNG DORSEY, M. D. is appointed Professor of Anatomy in the University of Pennsylvania, in the place of *Caspar Wistar*, M. D. deceased.

METEOROLOGICAL OBSERVATIONS.

State of the weather at Philadelphia, during the first six months of 1818.

January.

Thermometer—Lowest at 8, A. M. 11—31st day of the month,
Highest at 3, P. M. 48—7th.
Mean - - 32

Winds—variable, mostly westerly and southerly—little snow, the weather mild.

February.

Thermometer—Lowest at 8, A. M. 9—10th day of the month,
Highest at 3, P. M. 54—28th.
Mean - - 30.

Winds—chiefly westerly—The Delaware frozen over on the 8th, and continued fast until the 28th of the month; but little snow.

March.

Thermometer—Lowest at 8, A. M. 25—6th day of the month,
Highest at 3, P. M. 68—14th.
Mean - - 42.

Winds—westerly, and north and south—mild weather generally; little rain here; snow on the 28th; but very considerable inundations in the States to the northward and eastward; also in the north western parts of Pennsylvania. On the 18th, ice islands were seen off the banks of Newfoundland, and very tempestuous weather experienced on the French and English coasts.

April.

Thermometer—Lowest at 8, A. M. 40—20th day of the month,
Highest at 3, P. M. 64—16th.
Mean - - 50.

Winds—much northerly; cool weather; little rain. Account in the News-papers of an ice island being stranded on the most western of the Shetland islands, in the latter part of this month.

May.

Thermometer—Lowest at 8, A. M. 43— 4th day of the month,
Highest at 3, P. M. 84—27th.
Mean - - 60.

Winds—chiefly westerly; rain and cool weather until the 21st; from that time, fine growing weather. Unusually severe hail storms in Virginia.

June.

Thermometer—Lowest at 8, A. M. 64—19th day of the month,
Highest at 3, P. M. 92—28 and 30th.
Mean - - 75.

Winds—mostly westerly; copious rains. From the 1st to the 7th, many large ice islands seen between lat. 45. E. long. 46. and lat. 49. E. long. 50. The prospect of grain of all kinds is flattering, plenty of vegetables, and a moderate quantity of fruit. The weather unusually warm the last week of the month; an hail storm on the 18th. In the latter end of last year and beginning of this, many cases of Typhus gravior occurred, especially in the Alms House and Prison, which have disappeared, since the coming on of the warm weather; several severe cases of Rheumatism last month; Scarlatina, of a mild form, exists; some Small Pox continues, but does not appear to spread much, or to prove mortal often. A state of general health has been experienced, for the last six weeks; our common situation at this season when no epidemic prevails.

OBITUARY.

Died—in Philadelphia, on the 28th day of April, 1818, Doctor SAMUEL CLEMENT HOPKINS, aged thirty-one years.

———— on the 1st day of May, 1818, Doctor BENEDICT D. POTTS, in the twenty-eighth year of his age.

———— on the 31st day of May, 1818, Doctor ELIAS BOUDINOT STOCKTON, in the twenty-ninth year of his age.